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ON THE
MORTALITY OF CHILDBED
AND
MATERNITY HOSPITALS

THE PROPERTY OF THE N. YORK HOSPITAL

BY

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Obstetrical Society of Louisville, of the Imperial Royal Society of Physicians
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WHO CAN NOW,

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ADD TO HIS OTHER TITLES OF DISTINCTION

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PREFACE.

THE present work is, in some respects, premature. Questions are raised in it which cannot as yet be settled. But although in this aspect it is unripe for publication, yet it appears to the Author that some work of the kind is loudly called for at the present epoch. In times of revolutionary violence, when useful institutions are attacked, it is necessary to defend them with such weapons as can be quickly got. In science the only weapons available are truths and sound arguments. This book is an attempt to arrive at truths, and to use them in argument justly, so as to reach sound conclusions, and expose and destroy the unsound.

The work consists mainly of various essays composed during the recent discussions as to the value of Hospitals. The Author has, as might be expected, taken Maternity Hospitals as the field for his observations and arguments. But though an obstetrician naturally takes his facts from the statistics of Lying-in

Hospitals, yet the Author believes that they are, on the whole, best adapted for the discussion of the value of Hospitals generally. There is a unity about childbirth that does not exist in connection with amputations or any other surgical operations. Childbirths can be scrutinised in vast numbers. Their data have been collected without a view to any controversy. A lying-in woman is very liable to be affected by all kinds of insalubrious influences.

The Author was impelled to undertake the work by two considerations. Of these one was practical, the other scientific.

The practical consideration was the defence of Hospitals against unjust attacks. These invaluable institutions he found misrepresented in a manner which he considered most unfair, and which he knew to be most injurious. He felt bound to try what he could do, not so much in their defence as to secure them fair play.

The scientific consideration was the circumstance that a terribly erroneous statement by a most respectable author—Le Fort—was extensively quoted and applied, indeed made a text for several great tirades against Hospitals. This statement is that the mortality of women delivered at home is 1 in 212, and that the mortality of women delivered in Hospitals is 1 in 29. Such an erroneous and misleading statement, the Author felt, should not be allowed to go unchallenged.

The object of this book will not be gained if physicians and hospital-managers are induced by it to believe that their institutions are perfect, or even nearly so. He has nothing more at heart than to see improvements of all kinds—even radical changes, if necessary—introduced into our hospital system. His work is intended to add to the knowledge of physicians and hospital-managers, so that they may proceed with wisdom, and not be led by sensational or erroneous statements to be wildly revolutionary, of which there appeared to be some danger.

He has, in conclusion, to express his thanks to Dr. Angus Macdonald for valuable assistance in passing this book through the press.

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CHAPTER I.

THE MORTALITY OF CHILDBED.

THERE are some terms frequently used in this book which require some definition preliminarily. Childbirth implies parturition. The accidents of childbirth, or deaths from childbirth, are accidents or deaths arising from parturition. From these accidents and deaths, those of puerperal fever or metria are arbitrarily excluded. Childbed is a more general expression, implying the special conditions in a period of time, generally understood as of four weeks, extending from parturition, which it also includes, onwards for the puerperal or childbed month of lying-in. Childbed deaths include those from childbirth and metria. Mortality or deaths *of* childbed are those belonging to that state—*i.e.* childbirth and metria deaths. Mortality or deaths *in*, not *of*, childbed include all deaths in the four weeks of childbed. Deaths *in*, not *of*, childbed are all deaths, deaths from whatever cause, occurring within the four childbed weeks, including the period of labour.

To illustrate the use of these terms, I may take some examples. A death from unavoidable hæmorrhage is a childbirth death, and so is a death from rupture of the uterus, though it may be delayed for a week after parturition. A death from septicæmia or pyæmia, after delivery, is not a childbirth death, but a death from metria. All of these cases are childbed deaths. They are deaths *of* childbed. They are also deaths *in* childbed. But if a woman is poisoned by laudanum within four weeks after delivery, that is death *in*, not *of*, childbed.

These various definitions are not strictly natural or scientific, but they are here made for reasons which the reader will easily recognise as he goes on. They can scarcely be called new terms or new definitions of terms, for their meaning is such as is either already generally recognised, or is easily apprehended.

The mortality *of* childbed is a quantity not only not ascertained, but, so far as I can see, not at present ascertainable in a perfectly or even a nearly satisfactory manner. Yet it appears to me very desirable to make a definite, single-eyed attempt to approach as nearly as possible to a correct statement of this quantity. The result, if even moderately well established, cannot fail to be of immense value in contributing to the settlement of disputes as to the injurious or beneficent character of practices or of hospitals. I say only *contribute*, for much more is required for the purpose than a standard to measure by. But without some approach to a fixed standard, no progress can be made in discussions such as those alluded to.

In the recent animated debate on the value of Maternity Hospitals in the Dublin Obstetrical Society, more than one speaker set himself to answer the question proposed in this paper. I shall use much valuable information derived from that source;[1] but I think I have added considerably to it; and I have an advantage over the speakers there in this respect, that I am considering at present only this single point, "What is the mortality of childbed?" separate from the other questions raised in the famous debate.

Statistics are indispensable, and this is at present a very unfortunate circumstance; for, in addition to the well-known difficulties in using aright the coldest and simplest statistics, we are in the midst of much passionate struggling on the arena into which statistics are to be brought, and the heat of defence and attack is alone sufficient to induce much falling from strict logical sequence, without the addition of the temptation, in the same bad direction, offered by statistics. While I cannot claim for myself exemption from these dangers, I shall at least take the credit of trying to avoid them.

In the present question there are two great statistical difficulties. The first is, to decide upon the facts or circumstances to be compared. The second is, to get the facts or circumstances, after settling the first difficulty, as to what facts are to be got or are worth getting. Unless a thoroughly good understanding is arrived at on these points, the argument cannot advance a step; the quantity desired, the mortality of childbed, must remain unknown, and not even approximately fixed. We shall now inquire how they can be best settled.

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[1] *Dublin Quarterly Med. Journal*, August 1869.

First, What facts or circumstances settling or contributing to settle the quantity, the mortality *of* childbed, can be agreed upon as being worth obtaining and studying? Now, the Registrar-General gives us deaths of childbed, and he places them in two categories: first, childbirth deaths or deaths of childbed not arising from what is called metria; second, deaths from puerperal fever or metria. In the first category are placed deaths from rupture of the uterus, from puerperal eclampsia, from phlegmasia dolens, from puerperal mania, from placenta prævia, etc. In the second are placed deaths from metria or puerperal fever. This mode of arranging the deaths of childbed is very generally adopted, and at present I do not wish to make any theoretical objection to it; but a single statement is sufficient to show that the use of these two categories does not ensure the production of facts which the profession can agree upon and unite to accept: for the profession are not agreed upon the questions, What are childbirth deaths? and, What are metria deaths? Not only may obstetricians, well informed and strictly honest, differ as to which is the right category for a particular case; but there is also, and this is the great point, room for their differing as to a particular case being a childbed death (*i.e.* from childbirth or metria) or not. One class of practitioners may deliberately and honestly say of a case, This is not a childbed death (*i.e.* from childbirth or metria), while another class of practitioners may equally deliberately and honestly say of the same case, This is a childbed death (*i.e.* from childbirth or metria); and there is no means of always settling the question between them either scientifically or by authority. What, then, is to be done? The fact is, that all attempts at ascertaining scientifically or exactly the desiderated quantity, the mortality of childbed, must meantime be given up. There is no method of even getting facts upon the nature of which the profession are agreed. I could prove this by tedious references to writings of obstetricians of high authority, and by other arguments, but I believe it is quite unnecessary.

There are many valuable results obtainable, which, though not exactly what is desired, are very nearly so, and extremely useful, because the best obtainable with a view to guidance in great practical questions which demand an immediate answer of some kind—the best that can be got. Now, in the present instance, we can get the deaths *in* childbed indisputably, though not those *of* childbed; and there will be, in my opinion, no very great difference between the two quantities. The quantity wanted is the mortality *of* childbed (*i.e.* of childbirth and metria): it is unattainable. The quantity attainable is the mortality *in* childbed (*i.e.* of childbirth and metria, and every other influence producing a fatal result in the interval between the commencement of parturition and the end of the lying-in or childbed—that is, a period of four or six weeks, or any other time that may be agreed upon). It must be evident to all that this result can be got—namely, the number of women dying in the interval between the beginning of labour and, say, four weeks thereafter. As matters stand there is a difficulty, for we have no security as to the length of time included in the term lying-in or childbed. A month is the term generally adopted, but it is not settled whether this means twenty-eight, thirty, or thirty-one days. While we recognise this difficulty, yet we cannot doubt that this quantity is the best that can be fixed upon for observation, just because there can be no cavil about what it is—namely, the mortality *in* childbed from whatever cause, not the mortality *of* childbed (*i.e.* childbirth and metria).[1]

It will be observed that I have said that I do not believe there is any great difference between the mortality *of*, and the mortality *in*, childbed ; and the statement is indeed capable of demonstration. For the total four weeks' mortality of wives, or of women generally, at any child-bearing age, is a very small amount compared with the four weeks' mortality of wives in childbed, or of women in childbed generally ; and this very small amount is of course more than the amount of the deaths *in*, and yet not *of*, childbed, or, in other words, of the deaths not connected with childbed except by occurring during it. This embodies, I believe, a nearly correct scientific statement of the matter. But the subject requires to be otherwise looked at :

---

[1] On this subject see the *Lancet* for 1859, vol. ii. p. 213.

namely, as a practical matter of calculation; and here we find that there is an immense difference made by authors or statisticians between the deaths *in*, and the deaths *of*, childbed. It is to Dr. M'Clintock that we are indebted for the best elucidation of this practical side of the question, and I give his own account of this matter from his speech before the Dublin Obstetrical Society:[1]—

"A reliable estimate of the mortality among lying-in women confined at their own homes is a very great desideratum. I must honestly declare my conviction that up to the present time, notwithstanding our multiplied and elaborate Registration Reports, there is no reliable return of such deaths, and therefore it is no better than 'arithmetical idleness,' to be constructing out of these reports any standard of comparison between hospital and home midwifery practice: and this opinion is shared in by every medical man of experience who has bestowed any consideration on this matter. Nor have we to go far to discover the reason of this. The death of a woman in childbed, as every one here well knows, always attracts a great deal of attention, and is a fertile subject for popular comment and animadversion; but, if the cause of death is known to be puerperal fever—or anything pertaining thereto—then, indeed, quite a panic is created in the neighbourhood, and both doctor and nurse come in for more than their full share of blame. Hence, for their own reputation's sake, as well with the charitable motive of not alarming all the pregnant women in the community, the death is imputed to any other possible cause rather than to the dreaded puerperal.

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[1] *Dublin Quarterly Journal of Medical Science*, p. 266. See also p. 269.

"It is not necessary, however, that any motive be assigned for this. The defect lies in our system of registration—not in those who supply the returns. Practitioners make a very proper distinction between dying *in* childbirth, and dying *of* childbirth. When a woman happens to die in childbed of some intercurrent disease—as phthisis, pneumonia, dysentery, apoplexy, albuminuria, bronchitis, morbus cordis, etc.—this alone is returned, and rightly so, to the registrar as the cause of death. *Consequently all these deaths have no place in the registration reports of deaths in childbed. But a lying-in hospital is debited with every death occurring among its patients*—whether the death arise directly from parturition, from puerperal disease, or from any accidental intercurrent disease.

"In this way, I think, we can account for much of the discrepancy between the death-rate of lying-in hospitals, and that deduced from the returns of the Registrar-General. To illustrate my meaning, and to show how statistics must be influenced by this source of inaccuracy, I have compiled the following Table :—

TABLE I.

SHOWING DEATHS IN CHILDBED, CLASSIFIED UNDER THREE HEADS.

(*From Home Practice exclusively.*)

AUTHORITY.	Number of Cases.	Deaths from Accidents of Labour.	Deaths from Puerperal Diseases.	Deaths from non-Puerperal Diseases.	Totals.
Joseph Clarke . .	3,847	7	6	9	22
J. G. Crosse . . .	1,377	1	8	5	14
John Beatty . .	5,616	2	9	2	13
Thomas E. Beatty .	2,064	4	8	5	17
Churchill . . .	2,548	6	8	2	16
Browne[1]	...	22	6	7	35
M'Clintock . . .	652	0	4	2	6
Sir P. Dun's Maternity	336	...	...	2	2
Brunker	334	3	3	0	6
Totals . .	16,774	45	52	34	131

"In compiling this table, the results of private practice chiefly are taken, as being *thoroughly reliable*, and the patients having been all attended at home. Hospital statistics are excluded, because the great excess of metria cases in hospitals would necessarily disqualify them from showing the proportion that deaths from non-puerperal disease bear to all other deaths in childbed among patients out of hospital.

"From this table it is apparent that the deaths occurring in childbed from non-puerperal disease form considerably over one-fourth of all the fatalities. You can at once perceive the great effect which the omission of these deaths must have in lowering the death-rate of childbirth out of hospital. For example, when the deaths are returned as 1 in 200, they should, on the principle just laid down, be 1 in 150. Or, one other example: 11,722 English women died in childbirth in the years 1838, 1839, 1840, and 1841—the rate of mortality being one death to 171 births, or 168 women (after making the allowance for twins).[1] But if what I have stated be correct—viz. that only three-fourths of the gross number of deaths are registered—the true rate of mortality is found to be *one* in 126."[2]

---

[1] Dr. John Browne of Dundalk. The above results are from twenty years' practice, private and dispensary.

Having thus settled what can be agreed upon as obtainable with a view to this question, let us, *secondly*, inquire what sources we can look to for the desiderated data. Derived from any source, they will be imperfect in various ways. We must choose the best, the most trustworthy.

There can be no doubt that hospitals, with well-kept records, offer us data far more trustworthy than any other; and I believe this circumstance, while it ought to excite our admiration of them, has, on the contrary, been the prolific cause of much injurious slander. Whose character can endure or survive the divulging of the whole truth about it? One of the great difficulties in adopting hospital statistics arises from the early dismissal of the patients generally, the dismissal of some of the sick with a view to admission to other hospitals, and uncertainty thus introduced as to the number dying within a period longer than that during which all are indiscriminately retained in the institution. Like difficulties damage all other sources of data, and this similarity abates much of the consequent evil. The security of hospital statistics arises from their being compiled at the time of the facts emerging, from their being recorded by uninterested parties or without a view to any discussion, and from their being of undoubted truthfulness.

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[1] Fifth Annual Report of the Registrar-General for England and Wales.

[2] In justice to the returns of the Registrar-General, it must here be stated that they do not profess to include *all* the women dying in childbed, but only those dying from the effects or accidents of labour, and from puerperal diseases, which are included under the comprehensive term *metria.*

After hospitals, we turn, secondly, to the reports of the various Registrars-General. These have a certain and a very high value, arising chiefly from the largeness of the figures. This largeness, while it covers many errors, does not cover others. We have already said that we have no security that the deaths included under the designations "childbirth" and "metria" include all the deaths of either category; nor have we any security that both taken together include all deaths of childbed. They are intended to include all deaths *of* childbed, but not to give us any clue to the number of deaths *in* childbed, the quantity we hope to find out, or approximate to. Probably few omissions from these categories take place from a desire of the recording practitioner to conceal or delude; but no one knows how many may be omitted because the recording practitioner does not deem it right to record his case as one justly belonging to either category, or *vice versa;* and the practitioner cannot be found fault with, for he has no unexceptionable rule of guidance.

To show what sources of error in the registrars' reports are hidden from observation, I shall quote from Dr. Barnes a passage bringing one to light from a single locality:—"It is stated," says he, "in the Registrar-General's Report for 1856, that the mortality in childbirth in England and Wales in 1847 was 1 in 167, and that it had fallen to 1 in 227 in 1856. Now, having applied to Dr. Elkington for the puerperal statistics of Birmingham, I learn that the registrar of that town says that 'no one ever specifies the deaths in childbed or from puerperal fever!!!'"[1]

Trusting too implicitly to tables of mortality and registrars' reports, a student might be led to adopt such an absurd notion as that the mortality of childbed had been reduced from 1 in 167 in 1847 to 1 in 227 in 1856 in England and Wales. Like unfounded and improbable views as to the progress of midwifery have been so often repeated as to appear now to be generally believed. The paradox referred to, like many others, is based on statistics, and I shall not quote them, nor stop to show their worthlessness, for it must be apparent to all on a very little reflection. It is, on such deceitful grounds, asserted that between 1660 and 1820 the mortality of childbed in London fell from 1 in 44 to 1 in 107!!![1] In 1870, with our great registering machinery all at work, we cannot find out what is the mortality of childbed in London.[2] If we could, all the labour of obstetricians on the subject now under discussion might be spared. For my part, I think obstetrical common sense will be very contented if the true childbed mortality of modern London is at all less than in the London of 1660. It is very doubtful whether it is even now as low as 1 in 107.

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[1] See *Dublin Quarterly Journal of Medical Science,* vol. xxviii., 1859 p. 103.

I have already given Dr. M'Clintock's demonstration of the difference actually existing between registered deaths *of* childbed and those that occur *in* childbed.

The third source of data is a private search of the public registers, and the discovery, by this means, of all the women dying within a certain time, say four weeks, after childbirth. No doubt, in the statistics thus framed, there may be errors, but they will certainly all be errors of omission of deaths, from want of success in tracing them. The errors will all tend to make the mortality too small.

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[1] Simpson's *Obstetric Works*, vol. ii. p. 545. Merriman, who publishes the statistics referred to, points out their untrustworthiness. The births are got, says Merriman, by counting the christenings and the dead-born. The diminishing percentage of mortality is naturally accounted for by an increase of the number of children brought to the parish churches and registered, instead of being unchristened, or christened by dissenting ministers and not registered; while the dead mothers are supposed to be all registered, irrespective of the registering of the children. But the whole data are really so insecure as to be quite worthless.

[2] See Barnes, *Dublin Quarterly Journal of Medical Science*, vol. xxviii., 1859, p. 100.

The fourth source is the reports of out-of-door or home practice of dispensaries or hospitals. To them I attach no value whatever. I am well acquainted with the management of these institutions, and I know that the best of them present no data that I can dare to use in an inquiry like the present. The mortality which they record is often incredibly small. There is no security whatever that the deaths are recorded, whatever may be their cause. There is no security that theoretical views as to the nature of the cause of death may not completely destroy the value of the records. I could, from extensive experience of my own in dispensary work, adduce data which would indicate a figure of mortality that would be very delighting, if I could only believe it. Le Fort, in his well-known work, has fallen into this, which I must call, gross error, taking the data of the home practice of charitable institutions as reliable and fit for comparison with the data derived from hospital records. Doing so, he has, of course, arrived at extraordinary results.

Le Fort[1] says the mortality in hospitals or maternities is 1 in 29. What does this mean? It is, that, taking the data of all maternity hospitals together, he finds that 1 of every 29 delivered has died. I do not doubt it. But what is the value of this result, with a view to the question now before us, and I may add also (among others) before Le Fort? If the maternity department of the Hôpital de la Charité is so badly managed as to have a mortality of 1 in 7, what does that show with a view to the question of the mortality of childbed generally, or in hospitals as compared with that in homes? It is plain that it shows absolutely nothing with this view. It should, for mine and for Le Fort's purposes, be simply thrown aside out of view. I daresay an hospital could be so constructed and managed as to kill all its inmates. What of that, in the questions before Le Fort and myself? Will the addition of such data as are furnished by La Charité to such data as are got from all other hospitals, bad and good, such as that of Troyes (1 in 230), lead to any desirable result? In my opinion, to no result but confusion and darkness. Such statements as that of Le Fort, regarding the mortality in hospitals, only show how disgracefully mismanaged many hospitals are, how much need there is of the exertions of the philanthropist. Superabundant evidence can be adduced to show that it is easy to have far better results in maternity hospitals than 1 in 29; and it is well known that the best maternities are susceptible of vast improvements. Le Fort's labours show how bad they may be, and little more. They do not bring out what he and I want.

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[1] *Des Maternités*, Paris, 1866, p. 31.

Having made out the mortality of all hospitals to be, in fact, 1 in 29, Le Fort proceeds to investigate the mortality in home or dispensary practice, and he pursues with this the same method. But there are great differences in the two sets of data. In the case of hospitals, it is probable that the data are nearly what they pretend to be, nearly true. But in the case of the data of home practice, there is, in my opinion, not the least probability that they are, what they pretend to be, nearly true. The method of collecting the data of home practice renders them valueless. They are not got as the data of hospital practice are got.[1] But further, the data of M. Le Fort are, without doubt, themselves mutually destructive. We know that the mortality of different hospitals is extremely different. A bad one may have a mortality of 1 in 7; a good one a mortality of 1 in 100. But we have no reason to believe that such an extreme difference occurs in mortalities of home practice. Le Fort cites home practice (Stettin) having no mortality at all! Is this a valuable and instructive result? He cites home practice having a mortality of 1 in 595. Is this a valuable and instructive result? Is any one so foolish as to believe it? Will the jumbling of such figures together produce a result (1 in 212[2]) of any value? Errors heaped on errors produce only a more egregious error.

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[1] On this subject, see some remarks by Dr. Churchill, *Dublin Quarterly Journal of Medical Science*, Aug. 1869, p. 249. See also remarks by Dr. Kidd, *Ibid.* p. 242; also by Dr. Denham, p. 273.

Des Maternités, p. 33. I am astonished to find Depaul quoting

In the case of hospitals, we may get near the truth by studying one that is large enough and long enough established, and that has laws and conditions that are well known. In the case of the home practice of maternities, I know of nothing reliable as to mortality.

Lastly, there is another source of data—namely, private practice. But I regard it as a very questionable source. The reception of evidence derived from it is encumbered with difficulties. And there are some conditions of such data which I regard as to be always required before they are received as quite satisfactory. The first is, that the items or facts be written down at the time of their occurrence. The memory is a frail and treacherous source of statistics. The second is, that the data be not asked for by a second party known to have any object in view in their use; for such asking will inevitably lead, through the amiable qualities of the petitioned parties, to the production of data favourable to the petitioner's views, and the non-production of unfavourable data. In depreciation of the value of data derived from private practice, it is to be remembered that medical men are mortal, and have an indisputable tendency, and an inalienable right, to

Berard, approvingly, as the reporter of 1258 deliveries without a single death following, and this among the poorest of Paris. This fault is probably the result of mere thoughtlessness. If the statement of Berard is true, it surpasses anything known in any kind of practice; and the enemies of hospitals who adduce it are logically bound to commend, as favourable to recovery after delivery, the attendance of a student, and the immersion in all the loathsome peculiarities of the most wretched abodes of Paris. See *Fièvre Puerpérale;* Paris, 1858, p. 371.

say nothing of what looks like want of success. Suspicion naturally attaches to data remarkable for apparent success. This does not arise from any doubt as to veracity, but from the probability that practice, having apparently remarkable success, is published on that account. If the data of private practice could be got, they would be the best. But, as yet, no satisfactory data of this kind are procurable on a large scale.

We now proceed to the results afforded by the various sources above enumerated and considered.

The important result, let me repeat, which is sought, is the mortality, not *of* childbed (*i.e.* childbirth and metria), but *in* childbed (*i.e.* from all causes resulting in death within the period called that of childbed).

I.—Hospitals.

Some hospitals show an appalling, and, I may add, a disgraceful mortality. I could adduce a mortality of 1 in 3 in a certain period of the history of a great hospital.[1] From this climax, I could rise through successive stages of badness to a mortality that, so far as I can see, is nearly the present ordinary mortality in childbed. Let us take a well-known and well-managed hospital, and see what mortality it presents. It would be misleading to take all hospitals, for the bad would destroy the evidence of the comparatively good; and most are bad, many very bad. In order to get at the mortality in childbed, in such a rough practical way as we are now pursuing, it appears to me only to be necessary to take an hospital large enough and long enough established to give its statistical figures security against accidental interferences. I shall take the great Dublin Hospital.

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[1] *De la Fièvre Puerpérale*, etc. Communications à l'Académie Impériale de Médecine. Paris, 1858, p. 27.

During the seven years of Collins, 16,414 women were delivered, and 164 died; or in the proportion of 1 in 100.

During the three years of M'Clintock and Hardy, 6634 women were delivered, and 65 died; or 1 in 102.

During the seven years of Sinclair and Johnston, 13,748 were delivered, and 163 died; or 1 in 84.

II.—Reports of Registrars-General.

These are a great quarry for statistical data. They give the deaths *of* childbed. But it is only by some ingenuity that the deaths *in* childbed can be even approximately reached through them.

According to Faye and Schönberg, the mortality of childbed in Norway is 1 in 135.[1]

The mortality of childbed in Paris was 1 in 169 in 1861; 1 in 160 in 1862.[2]

The mortality of childbed in St. Petersburg is given from data supplied by Hugenberger as 1 in 149.[3]

The mortality of childbed in Dublin is given by Dr. Evory Kennedy as 1 in 114.[4]

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[1] See *Dublin Quarterly Journal of Medical Science*, August 1869, p. 270.

[2] Le Fort, *Des Maternités*, p. 33.

[3] *Ibid.*

[4] *Dublin Quarterly Journal of Medical Science*, vol. xlvii., 1869, p. 289.

The mortality of childbed in England and Wales is given by Farr as 1 in 189.[1]

The mortality in Edinburgh for the six years, 1860 to 1865, is 1 in 162.

The mortality of childbed in Prussia is said by Hoffman, calculating from 7,654,021 deliveries, to be 1 in 108.[2]

I need not give more of these statements. They show what deaths are returned to the public registers as childbed deaths. They give us the registrars' statements of the deaths of childbed. We have already given reasons for distrusting these statements, and we have also shown why the quantity sought should be the deaths *in* childbed, not *of* childbed.

Dr. M'Clintock has taken pains to find out how far the deaths in childbed exceed the deaths of childbed. He finds this quantity to be equal to at least one-third of the deaths of childbed; in other words, deaths in childbed, and not given as being of childbed, are at least one-fourth of the deaths in childbed. In his own words, "One-fourth is not at all too much to allow for the deaths omitted in the registration returns of deaths in childbed." In another place he says, "The deaths occurring in childbed from non-puerperal disease form considerably over one-fourth of all the fatalities."[3]

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[1] *Seventeenth Annual Report of the Registrar-General*, etc., *England*, 1856, p. 73.

[2] *Fifth Report of the Registrar-General for England*, 1843, p. 396.

[3] *Dublin Quarterly Journal of Medical Science*, August 1869, p. 267. I adopt M'Clintock's method, without strictly inquiring into the justice of his plan of handling the figures.

Correcting by this plan the data of childbed mortality just given, we have—

The mortality in childbed in Norway, 1 in 101.
" " Paris in 1862, 1 in 120.
" " St. Petersburg, 1 in 112.
" " Dublin, 1 in 86.
" " England and Wales, 1 in 142.
" " Edinburgh, 1 in 122.
" " Prussia, 1 in 81.

I cannot pretend to say what value I attach to these calculations. There is certainly a great want of precision about them. But that, for our present purpose, the registrars' reports require much correction, I am quite certain, and I shall here give an illustration of their faultiness. The Scottish Registrar's Report for 1855 gives a total of 118 deaths of childbed in Edinburgh and Glasgow. A private search made for me by experienced census clerks discovered, among the married women alone, 153 deaths within six weeks after delivery, in 1855.

III.—Private Search of the Public Records.

The only private searches of which I know are those by Tarnier and myself.

Tarnier examined the registers of the poor twelfth arrondissement of Paris, and found the mortality in childbed to be 1 in 322.[1] I have already laid down enough of well-considered figures to render this statement of the mortality in childbed highly improbable.

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[1] *Fièvre Puerpérale*, etc., par le Dr. S. Tarnier; Paris, 1858, p. 75.

No such smallness of mortality is pretended in any place well known; and Dubois, after pointing out the special difficulties of the search made by Tarnier, does not hesitate to throw discredit on it, and adds that, among the comfortable and well-to-do classes, private practice yields nothing like such a happily small mortality as Tarnier represents in the poverty of Paris.[1] The same statistic of M. Tarnier is unfavourably commented on by Danyau.[2] He had the death-rate of the same district investigated, and found in it a mortality of 1 in 60 from puerperal fever alone!

I had a careful search made in the Scottish registers for deaths in Edinburgh and Glasgow in the six weeks following the deliveries of the married women there, and I found 153 deaths in 16,393 deliveries, or 1 in 107. It may be supposed that the addition of a fortnight to the usual puerperal four weeks may account for much of this mortality. But this is not the case. The omission of the fortnight, or the confining of the search for deaths to a period of four weeks after delivery, would probably have made little difference in the result, for the fortnightly percentage of deaths among women of child-bearing age, and apart from the immediate influence of child-bearing, must be very small.

IV.—Private Practice.

In the years of my practice of which I have preserved records, I find 8 deaths in 736 cases, or 1 in 92.

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[1] *Fièvre Puerpérale*, etc.; Paris, 1858, p. 260.

[2] *Ibid.*, p. 402.

One of the fatal cases was not attended by me during labour, and may be omitted from the statistic. The mortality will then be 1 in 105. This is the mortality from all causes.

In a report of two years of his practice, Sir James Simpson[1] says he lost 4 cases in at most 180 deliveries; a mortality of 1 in 45. It may be supposed that this is the total mortality, but it is not expressly stated whether it is so or not. And the same is the case in some of the other examples from private practice which I shall give.

Dr. J. Clarke,[2] in 3847 deliveries had 22 deaths, or 1 in 174.

Dr. Crosse,[3] in 1377 cases had 14 deaths, or 1 in 98.

Dr. Labatt,[4] in 4368 cases had 26 deaths, or 1 in 168.

A London accoucheur,[5] in 2982 cases had 30 deaths, or 1 in 99.

Dr. M'Clintock,[6] in 652 cases had 6 deaths, or 1 in 108.

Dr. Brunker,[7] in 334 cases had 6 deaths, or 1 in 56.

Dr. Churchill,[8] in 2548 patients had 16 deaths, or 1 in 159.

Among 10,190 cases, a physician[9] had 107 deaths, or 1 in 95.

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[1] *Obstetric Works*, vol. ii. p. 642.

[2] Speech by Dr. M'Clintock. *Dublin Quarterly Journal* for August 1869, p. 268.

[3] *Ibid.* [4] *Ibid.* [5] *Ibid.* [6] *Ibid.* [7] *Ibid.* [8] *Ibid.*

[9] Merriman, *Difficult Parturition*, p. 320; where will also be found the reference for the practice of the London accoucheur.

Among 2064 cases, Dr. T. E. Beatty had 17 deaths, or 1 in 121.[1]

General Result.

Having led all the evidence I propose to adduce, I now attempt to draw a conclusion from it. I do so with much diffidence, yet with the full conviction that it is the best approximation to the truth that I can make.

NOT FEWER THAN 1 IN EVERY 120 WOMEN DELIVERED AT OR NEAR THE FULL TIME DIE WITHIN THE FOUR WEEKS OF CHILDBED.

At this result there need be no astonishment. How many women are delivered in circumstances unfavourable for recovery? Some mothers are immature. Many are diseased. Some begin child-bearing when old. All have to pass through the great risks involved in a first confinement. Some have excessive families. Some are confined under the murderously depressing influence of shame.[2] The accidents of childbirth are numerous—malpresentations, ruptures, eclampsia, floodings; obstetric operations are frequent. Puerperal fever is common.[1]

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[1] *Dublin Quarterly Journal of Medical Science*, August 1869, p. 299.

Hegar (*Archiv f. Gynæk.*, Bd. I. S. 192) states that among 34,553 cases attended by physicians and midwives in a district of Baden, were 253 deaths, or 1 in 137. This collection is valuable; but it is of a kind which I can scarcely class with any other. I therefore put it in a footnote.

[2] "I have already alluded (says Dr. M'Clintock, *Dublin Quarterly Journal of Medical Science*, p. 272) to the number of seduced or deserted women who seek the refuge and concealment of a large hospital. At the time of their lying-in, every one knows how peculiarly obnoxious these patients are to the fatal influences of labour and child-

Before concluding, I must observe that the mortality of any hospital or practice is not, of itself, a measure of success or of failure. It is quite possible that an hospital or a practice with a high mortality may be especially successful. For it may number among its items an extraordinary number of cases of danger and difficulty, and the figures may be so small that a very little addition to the deaths will have a very remarkable influence in increasing the average mortality in it.

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bed. During my seven years' mastership of the Rotunda (1854 to 1861), it came to my knowledge, without making any special inquiries regarding it, that 127 patients were unmarried women, and had come from every part of the country, and of these 31, or very nearly one-fourth, died in childbed, and chiefly from some form of metria."

[1] Le Fort (*Des Maternités*, p. 63) says that the statistics of private practice of several English accoucheurs have been published, and that their total mortality does not exceed 2 or 3 in 1000. This is vague enough, but I feel confident it is also quite incorrect. Le Fort gives no authority for his statement. I could adduce many more statistics of private practice, but as they do not change the view I have given, I do not encumber my pages with them.

CHAPTER II.

THE RELATION OF THE NUMBER OF THE LABOUR TO THE MORTALITY FROM PUERPERAL FEVER.[1]

THERE are two important questions regarding the mortality of lying-in women which certainly have not received the amount of attention which they deserve. They are interesting and important, not only in themselves, but also, in a high degree, on account of their bearing upon topics which are constantly discussed without taking into account the great light and influence which the answers to them might bring upon such topics.

The questions I allude to are:—Does the number of a woman's pregnancy regulate in any degree the mortality to be expected from lying-in? Does the age of the childbearing woman regulate in any degree the mortality accompanying this function?

Analogous questions in regard to some surgical operations and to some diseases have been discussed, and not without good results. This circumstance renders it the more extraordinary that the questions I have proposed regarding parturition have stimulated so little inquiry. The topics of childbearing and of its mortality, and particularly the indefinite disease puerperal fever, are among the most interesting and carefully studied in the whole range of medicine, and the neglect of the two questions named I can attribute only to the want of materials for their settlement. Yet I do not hesitate to say, that had the profession set these questions before them in their simplicity and importance, materials would ere now have been found or accumulated, and their most desiderated solution satisfactorily effected ere this time.

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[1] This and the four following chapters are nearly identical with some chapters in a work by the author, which has been for some time out of print, entitled *Fecundity, Fertility, Sterility*, etc.

I regret that at present I know of no data sufficient satisfactorily to decide the questions raised. Yet I shall lay before the profession such as I have collected. They are deficient in point of number and of precision. Had the numbers been much larger, the results would have had value in spite of the want of precision in the data as arranged for comparison. The element of want of precision consists mainly in the comparison of different pregnancies not being confined to women of the same age, or *vice versâ*. This condition is of course necessary to ensure against a probable source of error, the amount of which is unknown, consisting in the disturbing influence of age or of the number of the labour, upon mortality.

It is well known that a large amount of puerperal mortality is produced by that indefinite class of diseases unphilosophically and injuriously combined under the name of puerperal fever. So important is this class of diseases, that it appears to me worth while to discuss separately the influence of age on the mortality from them. The same should be done for all causes of death in or after labour as soon as data are collected. In the case of puerperal fever some data are at hand. It is not my object at present to enter on the vexed questions in reference to this erroneously so-called fever. No doubt many grave and cardinal errors prevail regarding this class of diseases, and statistics may contribute aid to demolish some of them. The invasion of this disease is well known to be described by a class of obstetricians as an "accident." To remove it from this category is a just object of ambition. To some extent this has already been effected by Sir J. Y. Simpson, who has shown that it is subject to the law of the duration of labour.[1] The object will be further promoted if it can be shown to be under a law of the number of the pregnancy, or of the age of the mother, or of both.

It may at first sight appear unnatural to enter upon a special kind of mortality in childbed, before describing the whole mortality in childbed. And it is worth while to consider this point briefly, to show that, at least with the means at my disposal, the influence of age is better and more securely observed in this special kind of mortality than in the whole mortality of childbed. For, in proportion as puerperal fever has the quality of an accident, as many obstetricians believe, so will the unalloyed influence of the number of the pregnancy upon its occurrence be observable. Were it evidently not an accident, but due to this cause or the other cause, it would be more and more difficult to eliminate the influence of such causes upon the mortality, with a view to arriving at the results produced by the number of the pregnancy. There are, specially in many first labours, such evident and direct causes of death in many cases, that the influence of the number of the pregnancy can make no alteration in the fate of the mother. Such cases, in however great numbers, can throw no light on the influence we are studying. In proportion as such cases are intermingled with others fitted to throw light on the subject, so will they obscure that light. Deaths in childbed from puerperal fever are, to some small extent, truly described as accidental: no cause for the supervention of the disease may be detected; just as this is the case, so will be the value of the testimony of such accidents to the influence of the number of the pregnancy.

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[1] *Obstetric Works*, vol. i. p. 530. See also *Edinburgh Medical Journal* for July 1857.

Before discussing generally the influence of the number of the different successive pregnancies, I shall compare, first of all, the influence of primiparity as compared with that of births after all subsequent pregnancies. It is well known that first pregnancies are, as a whole, attended by a much greater mortality than subsequent pregnancies, and this is a circumstance which scarcely demands explanation, for the primiparous woman has a longer and more difficult labour than others; many primiparæ are delivered under the influence of depressing mental emotions; in primiparous women all the arrangements, mechanical and other, for delivery are tested, and subsequent deliveries occur only in those who have so far successfully endured the trial as to survive. But it is particularly illustrative of the topic of this chapter to inquire what effect primiparity has in labours that are natural, in women against whose chances of recovery nothing is known, who have easily passed through their trials.[1]

Drs. Johnston and Sinclair, in their valuable work on *Practical Midwifery*, described 11,874 cases in which the labours were "purely natural." Of these 3699 were examples of primiparity. There were therefore, 8175 births after the first. Of the 3699 primiparæ, whose labours were purely natural, 20 died of puerperal fever. Of the 8175 natural deliveries in women who had already passed safely through the dangers of parturition, 21 were followed by puerperal fever and death. To compare these proportionally, among purely natural deliveries in primiparæ, every 185th woman died of puerperal fever, or ·57 per cent; while among similar deliveries in multiparæ only every 389th woman died, or ·25 per cent. Puerperal fever deaths are described by Messrs. Johnston and Sinclair as "considered accidental." Their interesting data show that primiparæ are very greatly more liable to this awful accident than others.

In order to illustrate this particular point, I have no other collection of cases of natural labour to refer to, and therefore nothing so valuable and directly applicable. But I shall adduce evidence derived from more general collections of cases, including all kinds of labour.

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[1] Kiwisch (*Klinische Vorträge*, I. Bd., iv. Aufl., S. 622) makes the following important statement regarding Dugés, but unfortunately does not subjoin a reference:—"Dugés (says he) maintains that primiparæ, and especially those who have passed the thirtieth year of life, are more liable to puerperal fever."

Professor Hugenberger of St. Petersburg has published some observations on this point made in hospital practice.[1] Of 2253 primiparæ, 97 died of puerperal fever. Of 5783 multiparæ, 141 died of the same kind of disease. Among the primiparæ puerperal fever death seized every 23d woman, or 4·35 per cent; while among the multiparæ death in the same form seized only every 40th woman, or 2·44 per cent.

Dr. Collins in his *Practical Treatise* describes 56 deaths from puerperal fever. Of these, 30 occurred among 4969 primiparæ, and 26 occurred among 11,445 multiparæ. Among the primiparæ, every 165th woman died of this disease, or ·6 per cent. Among the multiparæ, every 440th woman died of it, or ·23 per cent.

Among the married women whose deliveries were registered in Edinburgh and Glasgow in 1855 there were 58 puerperal fever deaths. Of these, 26 occurred in 3722 primiparæ, and 32 in 12,671 multiparæ. Of the primiparæ puerperal fever carried off every 143d, or ·7 per cent; of the multiparæ every 396th, or ·25 per cent.

Having shown, by the statistics already brought forward, that deaths from puerperal fever are among primiparæ at least twice as numerous as among multiparæ, I proceed now to inquire into the comparative mortality from this cause in labours following succeeding pregnancies.

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[1] "Das Puerperalfieber im St. Petersburger Hebammen-Institute von 1845-1859." S. 24. Separat-Abdruck aus der *St. Petersburger Medicinischen Zeitschrift.* For some further statistics pertinent to the question, see the *Klinik der Geburtskunde.* Von Dr. C. Hecker und Dr. L. Buhl. S. 226.

Hugenberger devotes a short paragraph to this topic, and gives interesting data, which I here produce in a tabular form:—

TABLE II.

Showing the Mortality from Puerperal Fever in different Pregnancies.

(*From Hugenberger.*)

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage of Deaths.	Or one in
1st . . .	2253	97	4·30	23
2d to 4th .	4031	85	2·11	47
5th to 9th	1563	47	3·01	33
10th to 19th	189	9	4·76	21

This table of Hugenberger's data justifies his remarks. He says that the greater or less frequency of previous pregnancies appears to be not without influence upon the lying-in; for while those pregnant from the second to the fourth time show the most favourable results, the first increment of mortality begins with those in the fifth to the ninth pregnancies; and a greater mortality still is observed in women in the tenth to the nineteenth pregnancies. If we could dare to adopt as demonstrated what Hugenberger's data seem to show—and as yet I have adduced nothing calculated to shake their evidence—we should have an extremely interesting addition to our knowledge of the influence of the number of the pregnancy upon the danger of confinement. It would appear that from the very great danger of a first confinement, the woman passed into a period of comparative safety in the next succeeding confinements, till she came to about the fifth lying-in, when danger began to increase; and as pregnancy succeeded pregnancy, danger still further increased, until it reached a degree as great as that of a first confinement.

An interesting contrast of these results with what is known of the fecundity of women at different ages may be made. The average age of wives in Edinburgh and Glasgow bearing first children is 24 years. The average age of wives bearing fifth children is 31 years. From the 25th to the 30th year women are more fertile than at any other time. It is within the ages of 25 to 30 that are included the average ages of women bearing second, third, and fourth children, those produced with least danger to life. Hence, if the data are good and sufficient, there is a coincidence between the time of the greatest amount of safety and that of the greatest fecundity; and diminished fecundity, or likelihood of having children, occurs when danger is great; that is, in first pregnancies and in fifth and subsequent pregnancies, or in pregnancies of women below 25 years of age and above 30. But this point will be better and more directly demonstrated when the influence of age is itself discussed.

I shall now bring forward other data similar to Hugenberger's, with a view to observing whether they confirm his results or not.

In Edinburgh and Glasgow in 1855 there were 58 puerperal fever deaths of wives, all occurring before the ninth pregnancy. There were in that year delivered 15,384 wives pregnant for the eighth time or less. Arranging these according to the number of the pregnancy, we have the following

TABLE III.

SHOWING THE PUERPERAL FEVER DEATHS OF WIVES DELIVERED IN EDINBURGH AND GLASGOW IN 1855.

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage of Deaths.	Or one in
1	3722	26	·698	143
2	2893	8	·276	361
3	2534	11	·434	230
4	1982	6	·303	330
5	1543	2	·129	721
6	1221	2	·164	610
7	848	1	·118	848
8	641	2	·312	320

This table is scarcely a fit object of comparison with Hugenberger's, for it will be observed that while his table has cases of death in women even in the nineteenth pregnancy, no wife died after delivery in Edinburgh and Glasgow in 1855 whose pregnancy was above the eighth. So far as this imperfect table goes, however, it is somewhat in opposition to the general tenor of the results published by Hugenberger.

In the work on practical midwifery by Johnston and Sinclair I find a table of 75 puerperal fever deaths, in 74 of which the number of the pregnancy is given. Unfortunately, I can discover in the work no data regarding the number of the pregnancies of the whole women delivered. Unwilling, however, to lose any advantage that may be gained from the table of pregnancies of 74 puerperal fever deaths, I have in the following table arranged them for comparison with the whole women delivered in Edinburgh and

TABLE IV.

SHOWING A COMPARISON OF PUERPERAL FEVER DEATHS IN THE DUBLIN HOSPITAL WITH THE NUMBER OF PARTURIENT WIVES IN EDINBURGH AND GLASGOW IN 1855.

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage of Deaths.	Or one in
1	3722	40	1.07	93
2	2893	6	.27	482
3	2534	11	.43	230
4	1982	3	.15	661
5	1543	3	.19	514
6	1221	4	.32	307
7	848	2	.23	424
8	641	0		
9	425	3	.70	141
10	222	1	.45	222
11	152	1	.60	152

Glasgow in 1855. Of course the percentages derived from this comparison are not figures of actual value, but only of value for comparison with one another; and it is interesting to observe that they roughly confirm the results of Hugenberger. After a great mortality in first pregnancies, there is a great improvement in second, third, and fourth pregnancies; and then, again, as the fifth pregnancy is passed, the mortality rises as the number of the pregnancy increases. It must be admitted that this accordance is not very exact, the regularity of the results being disturbed by the great mortality in third pregnancies, and the absence of mortality in eighth pregnancies. There can be no doubt that the value of the table is not very great; yet it evidently points towards confirmation of Hugenberger. Larger and better data are required to produce a satisfactory assurance.

CHAPTER III.

THE RELATION OF THE NUMBER OF THE LABOUR TO THE MORTALITY ACCOMPANYING PARTURITION.

In pursuing this topic I shall follow the same course as I observed in describing the mortality from puerperal fever, beginning by a comparison of the mortality of first labours with that of all subsequent labours.[1]

The first data which I adduce are Johnston and Sinclair's 11,874 cases of purely natural labour. These are specially valuable for the purpose, for nearly the very reasons which enhanced their value when puerperal fever was the only cause of mortality under consideration—reasons which, therefore, need not be repeated here. Of 11,874 purely natural cases, 3699 were first labours, and 8175 subsequent labours. Of the primiparæ 33 died, and of the multiparæ 34, or 1 in 112 of the former, or ·89 per cent, and only 1 in 240 of the latter, or ·41 per cent.

These purely natural cases form part of a total of 13,748 labours described by Drs. Johnston and Sinclair. Of these, 4535 were primiparæ, and 9213 multiparæ. Among the former 83 died, or 1 in 54, or 1·8 per cent, and among the latter 80 died, or 1 in 115, or ·86 per cent, a mortality, it is to be remarked, relatively almost the same as among the purely natural labours.

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[1] On this subject see some valuable remarks by Dr. Barnes, in the *London Obstetrical Society's Transactions*, vol. i. p. 311 ; and some data, with remarks, by Hegar, *Archiv f. Gynæk.* Bd. I. S. 192.

We may now take those labours alone which were not purely natural. Of these, 836 were in primiparæ, and of these 50 died, being 1 in 17, or 5·98 per cent; while 1037 were among multiparæ, and of these 46 died, being 1 in 22, or 4·43 per cent. Here it is at once observed that the relative mortalities are nearly alike, forming a striking contrast to the relative mortalities under any other circumstances. It is unfortunate that this striking observation is founded on so few data. It cannot fail to excite reflections in the practical obstetrician. Such would be out of place in this book, and I shall only diverge so far as to remark that here the primiparæ evidently hold such a greatly improved position, that while in natural labour puerperal fever carries off proportionally twice as many primiparæ as multiparæ, and that while in labours generally, twice as many of primiparæ die as of multiparæ; yet in unnatural labours the balance is restored, the primiparæ escaping the special danger conjoined to all the evils connected with primiparity, nearly as often as multiparæ escape the special danger alone, without the evils which all the statistics hitherto adduced show to attend primiparity.

In Dr. Collins' *Practical Treatise* the deliveries of 16,414 women are described. Of these, 4969 were primiparæ, and 11,445 were multiparæ. Among the whole there occurred 164 deaths, but the number of the pregnancy is given only in 160 cases. Of these 160 deaths, 80 occurred among the primiparæ, being 1 in 62, or 1·61 per cent; and 74 occurred among the multiparæ, being 1 in 155, or ·64 per cent.

In the work of Messrs. Hardy and M'Clintock on Midwifery and Puerperal Diseases, 6635 cases of delivery are described. Of these, 5852 are described as natural deliveries. Among them were 1752 first labours, and 4100 subsequent labours. In the former the deaths were 7, being 1 in 250, or ·4 per cent; in the latter 9, being 1 in 455, or ·22 per cent.

The whole cases in the work of M'Clintock and Hardy are, as already said, 6635. Of these 2125 were in primiparous women, and 35 died, being 1 in 60, or 1·65 per cent. Among multiparæ were 4510 deliveries and 30 deaths, being 1 in 150, or ·66 per cent.

In Edinburgh and Glasgow in 1855 there were 16,393 deliveries of married women. Of these, 3722 were in first labours, and 50 died within six weeks after delivery, being 1 in 74, or 1·34 per cent. The multiparæ numbered 12,671, and of these 103 died, being 1 in 123, or ·81 per cent.

Having thus compared the mortality of primiparæ with that of all other parturient women, I proceed to inquire into the mortality of each successive pregnancy.

The accompanying table is made from the Edinburgh and Glasgow registers for 1855. It exhibits the number of wives delivered in each successive pregnancy, their mortality, and the percentage of mortality to deliveries. Casting the eye along the percentage column of this table, one does not discover any marked indication of a regular variation after the great mortality of primiparæ is passed.

TABLE V.

Showing the Mortality among Wives delivered in Edinburgh and Glasgow in 1855, in each Successive Pregnancy.

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage.	Or 1 in
1	3722	50	1·343	74
2	2893	24	·829	120
3	2534	25	·986	101
4	1982	13	·655	152
5	1543	13	·842	119
6	1221	7	·573	174
7	848	7	·825	121
8	641	8	1·248	80
9	425	3	·706	142
10	222	1	·450	222
11	152	1	·658	152
12	61			
13	34			
14	11			
15	6	1	16·666	6

I have no other similar exact data to add to what is given in the last table (V.) The authors from whom I derive the following data as to the pregnancies of women dying after delivery do not give the numbers of the pregnancies of all their cases with which to compare the pregnancies of those that died. But I here make the data regarding deaths available by comparing them with the pregnancies of the whole wives delivered in Edinburgh and Glasgow in 1855. This composite table will thus not give results or percentages representing actual values, but only results for mutual comparison, and I venture to think they are valuable. The table is prepared as follows:—The first column states the number of the pregnancy; the second gives the number of wives delivered in Edinburgh and Glasgow in 1855, in each successive pregnancy; the third column gives the number in each successive pregnancy of a collected mass of cases

TABLE VI.

SHOWING A COMPARATIVE PERCENTAGE OF DEATHS IN SUCCESSIVE PREGNANCIES.

No. of Pregnancy.	No. of Mothers.	No. of Deaths.	Percentage.	Or 1 in
1	3722	254	6·82	15
2	2893	60	2·07	48
3	2534	64	2·52	39
4	1982	39	1·97	51
5	1543	31	2·01	49
6	1221	28	2·29	43
7	848	16	1·88	53
8	641	15	2·34	42
9	425	13	3·06	32
10	222	9	4·05	24
11	152	5	3·28	30
12	61	1	1·64	61
13	34	4	11·77	8
14	11			
15	6	1	16·66	6

of childbirth deaths; the fourth column gives the percentages of these deaths in the deliveries in each successive pregnancy. In the third column are given 540 deaths gathered with care from the following sources:—153 from the Edinburgh and Glasgow registers above referred to; 160 from a table in page 364 of the *Practical Treatise* of Dr. Collins; 162 from the *Practical Midwifery* of Drs. Johnston and Sinclair; and 65 from the work on *Midwifery and Puerperal Diseases* of Drs. M'Clintock and Hardy.

This last table appears to me to show with considerable force that, after a woman has passed her ninth pregnancy or thereabout, she comes gradually into more perilous child-bearing, danger increasing with every unit added to the number of her children. To collate with this, it is interesting to the obstetrician to note what has been already shown, that, after the ninth, pregnancies recur with greater rapidity than before it.[1]

Having now led all the evidence I intend to adduce, I shall, in conclusion, add a few general and recapitulatory remarks.

First of all, it must be noted that I have, hitherto at least, said nothing regarding the nature of the relation between the number of the delivery and the mortality attending it. It is true the data recorded demonstrate more or less completely certain coincidences, which may be called laws. But they establish nothing further. These laws are as follows:—

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[1] See the author's work on *Fecundity, Fertility, and Sterility*, chap. x.

1. The mortality of first labours is about twice the mortality of all subsequent labours taken together.

2. The mortality from puerperal fever following first labours is about twice the mortality from puerperal fever following all subsequent labours taken together.

3. As the number of a woman's labour increases above nine, the risk of death following labour increases with the number.

4. As the number of a woman's labour increases above nine, the risk of death from puerperal fever following labour increases with the number.

5. If a woman have a large family, she escapes extraordinary risk in surviving her first labour, to come again into extraordinary and increasing risk as she bears her ninth and subsequent children.

These laws, although they merely state coincidences, have very important practical bearings, which are too self-evident to require description. They have also important pathological bearings. The most important, perhaps, of these relate to puerperal fever. These also I shall not enter upon further than to say that the occurrence of puerperal fever specially among primiparæ, and women who have borne large families, —its pretty close correspondence in relative amount to the general mortality of parturition after different pregnancies—its subjection also to the law of the duration of labour,—do not appear to me to lend support to the views hitherto generally entertained regarding it, and expressed in the words accidental, fever, contagious, epidemic. Another point under this head I shall merely mention. Authors, comparing the mortalities of lying-in institutions, whether from puerperal fever or from other causes, are frequently found neglecting to begin by ascertaining whether or not they are fit objects of comparison, and under this head, *inter alia*, neglecting to ascertain the comparative amount of primiparity in each institution. It is plain that, unless there be nearly the same comparative amount of primiparity in the institutions, their respective gross mortalities cannot be justly contrasted with one another.

The well-known protraction of labour in primiparæ may to some appear a sufficient cause of the increased mortality of first child-bearing. But mere prolongation of labour for a few hours cannot, in my opinion, be regarded as any satisfactory explanation of the causation of this increased mortality. In one set of Johnston and Sinclair's cases, the labours of primiparæ are called purely natural, and they are compared with similar purely natural cases in multiparæ; and the mere addition of a few hours to the length of labour in such primiparæ is not a sufficient cause of their mortality being twice as great as that of similar multiparæ. Denman alludes to "a vulgar and pernicious error which makes no distinction between the slowness and the danger of a labour." It would be to fall into this error to explain the increase of mortality merely by increased length of labour.

It must be held as proved, that according as labour increases in length, so the mortality accompanying it increases; and that this is true not only of the whole mortality, but also of the special mortality from puerperal fever. This law, although it must have weighty bearings on the mortality of primiparæ with their long labours, cannot be regarded as to any great degree throwing light on it; for we find new increments of mortality after the ninth labour, when we have no reason to believe that labour is more prolonged than in labours preceding the ninth, in which the mortality is less. In other words, we have the number of the labour denoting increase of mortality where there is no evidence of accompanying increase of its duration. The law of duration, then, does not enable us to explain the variations of mortality in different labours.

To completely exclude the influence of the law of duration would be very desirable; but we see no present prospect of doing this, except by processes of reasoning. Without such, it could only be done by comparing a series of labours of different number, but in all which the duration was the same.

It must be remarked that the law of duration certainly has important bearings on the data and arguments herein adduced to show the influence of the number of the labour, and that the extent of these bearings is undecided. At the same time it is equally sure that the law of the number of the pregnancy has important bearings on the data and arguments adduced to show the influence of the duration of labour, and the extent of these bearings is undecided.

The mutual influence of the data and arguments in these demonstrations must be great, and it remains for future observers to accumulate materials for either showing the amount of these influences, or for a separate demonstration of the laws by data which do not intermingle them in their conditions.

It is worth while to remark that, restricting for a moment our regard to the great mortality of primiparæ (exceeding as it does that of multiparæ, taken together), we have a set of cases—those of Johnston and Sinclair—where the deaths were from puerperal fever, and in which the average excess of duration of labour in primiparæ above that observed in multiparæ was 4 hours. In multiparæ the average duration was said to be 8 hours; in primiparæ 12 hours. Looking at this increased duration, and the correspondingly increased mortality in primiparæ, with the light thrown on it by tables published by various authors to demonstrate the law of duration, it appears to me that the increase of mortality in primiparæ is above that which these tables appear to give as the increase corresponding to a rise in duration from an 8-hours' labour to a 12-hours' labour.

These various remarks I have made with a view to keeping the demonstration of the influence of the number of labour on childbed mortality in its proper light, to keep it separate from other laws or supposed laws with which it may be confounded. I have alluded, with this view, to the causation of the variations of mortality according to the number of the pregnancy.

It is no main part of this chapter to enter on this subject, but a few words may not be out of place. It would be foolish to imagine that any injurious influence or the reverse could spring from the mere number of the pregnancy. A woman in a first may, and often does, have as fortunate a delivery as in any other. To ascribe to the number of pregnancy any potency would be to fall into the error of those students of the duration of labour who ascribe great potency to the mere addition of length to a labour. In the case of the law demonstrated in this part, and in the case also of the law of the duration of labour, it appears to me that the source of the variations of mortality is to be looked for in the introduction of complications. I here use the word complications in a much wider sense than is generally ascribed to it, wishing it to imply injuries or injurious tendencies far slighter than those ordinarily classed as complications of labour. I have no doubt that all of these, however minute or slight, have their weight in giving proclivity to a fatal termination of the childbed. Puerperal fever may have its root in an otherwise insignificant perineal laceration, as well as in a phlebitis or endometritis.[1]

In primiparæ, as labour goes on, complications occur which are not nearly so liable to attack a woman in her next subsequent labours. These have their origin in various sources, chiefly in mechanical difficulties, and these often so slight as not to take the case from the category of purely natural, in an arrangement where the labour is alone taken into consideration, to the exclusion of the childbed.

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[1] On this subject see remarks by Schroeder. *Schwangerschaft, Geburt.* etc., S. 197.

Multiparæ are specially and increasingly liable to complications of a different kind connected with constitutional diseases, and with local infirmities of the uterus.

This introduction of complications forms also the main explanation of the law of the duration of labour.[1] Indeed, in a rough way, it may be held that the statement of duration is a statement of the increase of complications; for it is known that as labour lengthens out, so complications increase in frequency. Without these complications duration would be of small importance, as the profession has generally held. Their introduction is present evil and the seed of future disasters. Tables have been framed to show the increasing introduction of complications as labour is prolonged, but I only refer to them. They are utterly useless, so far as I know them, because they are founded only on an enumeration of those of the graver sort. Further, the introduction of complications is not ruled exclusively by the duration of labour. Many are rather connected with precipitate parturition. The complications which probably contribute largely to produce the increased fatality of labours after the ninth are not all included, or capable of inclusion, in any statement of duration, being present before and after the process.

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[1] See a mass of important and pertinent facts tabulated and discussed by Simpson. *Obstetric Works*, vol. i. p. 430.

CHAPTER IV.

THE RELATION OF AGE TO THE MORTALITY FROM PUERPERAL FEVER.

On this subject important information is to be found in a letter addressed by Dr. Farr to the English Registrar-General, and published in the appendix to that officer's seventeenth annual report:—

"What (says Dr. Farr) is the danger of death by childbirth among women of different ages who bear children during the year? This is a different question, which is of practical importance both in medical science and in the business of life insurance. The defect in the English schedule, which, as yet, contains no column for the ages of the parents of the children registered, renders it impossible to answer this question with precision. It will, however, be useful to obtain an approximate answer; and this we have been able to give by determining the probable proportion of women who bear children at each age, from the Swedish returns, and by applying the fraction expressive of this proportion to the English women living in 1851 at the corresponding age, the probable number of them who became mothers every year is determined. The total number thus determined for the year 1851 is

609,845 ; while the actual average number of the births in the seven years by the returns was 603,045. It is thus evident that the estimate differs to no great extent from the facts, and it may be assumed that the births corrected for twins, triplets, and still-born children in England, would represent nearly 609,845 child-bearings."[1]

The following table, extracted from the data supplied by Dr. Farr, shows the mortality from puerperal fever in four decenniads :—

TABLE VII.

SHOWING THE MORTALITY OF CHILDBEARING WOMEN FROM PUERPERAL FEVER, IN ENGLAND, AT FOUR DIFFERENT AGES (FARR).

Ages . . .	15-24	25-34	35-44	45-54
Childbearing women	107,440	328,720	166,140	7545
Deaths from puerperal fever . .	298	486	256	12
Percentage . .	·277	·148	·154	·163
Or 1 in every . .	360	676	649	628

The large figures in this table give great value to the result, that while childbearing women aged from

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[1] I have been repeatedly consulted by the medical officers of insurance offices as to the proper conduct of cases of application of women for insurance who were childbearing, or had the prospect of childbearing in future. Any advice I have hitherto given has been in very general terms. In this volume, and in my work on *Fecundity*, etc.,

15 to 25 do die of puerperal fever in a proportion far exceeding that of women at any other age, the child-bearing women aged from 25 to 35 are carried off by the same disease in the lowest proportion compared with all others. Puerperal fever mortality, at its lowest among the lying-in aged from 25 to 35, rises on either side of this age, but it rises far more quickly and highly as age decreases than as age advances.

It would be unphilosophical to draw from this table even a presumption as to the influence of age on puerperal mortality, until careful consideration has been made of all the influences besides age which may have a bearing on it. Now, as far as I know, the paramount influence interfering with deductions from this table as to the influence of age is that of the number of the labour. Of the influence of primiparity, Dr. Farr, Dr. Tyler Smith (*Manual of Obstetrics*, chap. xlviii.), Dr. Barnes, and Dr. Stark, have had some degree of appreciation. But Dr. Hugenberger has, in some data he has published, actually separated the primiparous from the multiparous, with the view of eliminating this great influence. I here produce the tables of Hugenberger, re-arranged for uniformity's sake (VIII. and IX.)

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there is now given a basis from which the actuary may calculate the answers to the most important questions in this topic. He can determine the fecundity of the female, or her chance of having offspring; the fertility, or the number she is likely to have; the time when she will probably become relatively sterile; the risk of death in bearing her first child; and if she survives the birth of her first child, the risk of death in her subsequent confinements.

TABLE VIII.

SHOWING THE MORTALITY OF PRIMIPARÆ, OF DIFFERENT AGES, FROM PUERPERAL FEVER, IN THE MIDWIVES' INSTITUTE OF ST. PETERSBURG. (HUGENBERGER.)

Ages	15-18	19-22	23-26	27-35	36-45
Childbearing women .	147	859	711	495	41
Deaths from puerperal fever	7	25	22	39	4
Percentage . . .	4·76	2·91	3·09	7·88	9·75
Or 1 in every . . .	21	34	32	13	10

TABLE IX.

SHOWING THE MORTALITY OF MULTIPARÆ, OF DIFFERENT AGES, FROM PUERPERAL FEVER, IN THE MIDWIVES' INSTITUTE OF ST. PETERSBURG. (HUGENBERGER.)

Ages	18-22	23-26	27-35	36-53
Childbearing women . .	503	1410	2967	903
Deaths from puerperal fever	11	29	74	27
Percentage . . .	2·18	2·05	2·49	2·99
Or 1 in every . . .	46	48	40	33

These tables are interesting, and seem to show that Hugenberger felt the necessity, in the study of the bearing of age on puerperal fever mortality, of separating primiparæ from multiparæ. Any special results which might be drawn from them I think little worthy of consideration, in deference to the much larger and more valuable data which I adduce, and on account of the extraordinary mortality which the tables reveal.

It may with truth be said that, to make a perfectly satisfactory comparison of the mortalities of women of

TABLE X.

SHOWING THE MORTALITY OF CHILDBEARING WOMEN FROM PUERPERAL FEVER IN ENGLAND, AT FOUR DIFFERENT AGES, CORRECTED FOR PRIMIPARITY.[1]

Ages . . .	15-24	25-34	35-44	45-54
Childbearing women	107,440	328,720	166,140	7545
Deaths from puerperal fever corrected for primiparity . .	194½	339½	256	12
Percentage . .	·181	·121	·154	·159
Or 1 in every . .	552	823	649	629

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[1] It is to be remarked that this and the following tables, corrected from Farr's data, give results for different decenniads that may be compared only with one another. The table would not give actual values even were Farr's data actual values, which they are not. Correction has been made only in the line of deaths by taking away one-half of the deaths of primiparæ. This makes the table read as if a table of multiparæ. This proceeding, being simpler, has been preferred to another, which might have been followed—namely, to extract from the mothers the whole primiparous by estimate, and to extract from the deaths those of primiparæ, and compare the remaining multiparæ and deaths of multiparæ.

different ages, it is necessary to compare with one another masses of women of different ages in each successive pregnancy. I know of no data for this purpose. Hugenberger's data of primiparæ are a poor instalment, and my own Edinburgh and Glasgow data are equally insufficient, and I do not think it necessary to encumber these pages with them.

I have, however, ventured to increase the value of Farr's data, with a view to the question of the influence of age, by the following method. In Table X. the result is given.

The correction for primiparity is made because the puerperal fever mortality after first labours is at least double the puerperal fever mortality of all other labours taken together. In order to remove entirely, or almost entirely, the disturbing influence of primiparity, then it is necessary to turn out of the data one-half of the deaths of primiparæ. The number of puerperal fever deaths of primiparæ at different ages is got by determining their probable proportion from the Edinburgh and Glasgow mortality of 1855.[1] The

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[1] TABLE XI.—Showing the Mortality from Puerperal Fever of Primiparæ in Edinburgh and Glasgow in 1855.

Ages	15-19	20-24	25-29	30-34	35-39	40-44
No. of primiparæ . .	331	1859	1007	354	134	33
Deaths by puerperal fever	2	14	7	3		
Percentage	·604	·753	·695	·847		
Or 1 in	165	133	144	118		

preponderance of primiparæ at the earlier ages renders this correction necessary, and I only regret that the smallness of the data prevents us from ascribing to the correction a high value.

It may with truth and with some cogency be said that Farr's table should be further corrected for the increased mortality accompanying ninth and subsequent pregnancies which fall into the more advanced ages.[1] I do not attempt this correction, because it

TABLE XII.—Showing the Number of Children Born in First and Subsequent Pregnancies in Edinburgh and Glasgow in 1855, and the Average Ages of the Mothers in each Successive Pregnancy.

No. of Pregnancy.	No. of Children.	Average Age of Mother.
1st	3722	24·6
2d	2893	26·2
3d	2534	27·6
4th	1982	29·9
5th	1543	31·5
6th	1221	32·9
7th	848	34·9
8th	641	36·1
9th	425	37·5
10th	222	38·8
11th	152	39·2
12th	61	40·0
13th	34	41·7
14th	11	42·4
15th	6	42·7
16th	2	48·5
17th	2	41·5
18th	1	40·0
19th	1	48·0

cannot, with the means at my disposal, be done satisfactorily. But the omission of this correction will, comparatively, cause little inaccuracy in the results drawn from the table; for births in ninth and subsequent pregnancies are proportionally few, and the average age of women in ninth and subsequent pregnancies is above thirty-seven years, an age before which the injurious influence of elderliness appears to have already shown itself.

It has, lastly, only to be remarked that reference to Table X., corrected as it is for primiparity, shows results still closely resembling in general features those derived from the uncorrected table. Though similar in general features, there is evidently great difference in the numerical variations in the two Tables (VII. and X.); and I think there can be no doubt that the last table (X.) gives an approximation to a view of the influence of age far more faithful than the first (VII.)

CHAPTER V.

THE RELATION OF THE AGE OF THE MOTHER TO THE MORTALITY ACCOMPANYING PARTURITION.

THE first table which I shall adduce under this head is extracted from the data of Dr. Farr, already referred to. The calculations, as made by Dr. Farr, give the mortality according to age, but since primiparous females are included in the lists, they are of little value as indicating the influence of age. I have, as in the table of puerperal fever deaths, corrected Dr. Farr's data for primiparity, and in the penultimate line given the percentages; which may be held as showing, when compared one with another, an approximate estimate of the influence of age on the mortality of parturition.[1]

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[1] See some valuable and corroborative statements by Hegar, *Archiv f. Gynæk.*, Bd. I. S. 192.

TABLE XIII.

SHOWING THE MORTALITY OF CHILDBEARING WOMEN IN ENGLAND AT FOUR DIFFERENT AGES, AND THE SAME CORRECTED FOR PRIMIPARITY.

Ages	15-24	25-34	35-44	45-54
Childbearing women	107,440	328,720	166,140	7545
Deaths	718	1,397	1,051	66
Percentage	·668	·425	·633	·883
Deaths corrected for primiparity	473	1,216	1,033	66
Percentage	·440	·369	·621	·875
Or 1 in every	227	270	160	114

Here the large figures give a proportionate value to the results. The women aged from 25 to 34 have the fewest deaths among them—namely, ·369 per cent. They are more fortunate than the very young women aged from 15 to 24 by ·07 per cent. But the seeming influence of this youthfulness in aggravating the danger of women is slight when compared with that of advancing years, the decenniads following that from 25 to 34 showing a mortality increasing in a far higher ratio.

The next table which I adduce is made from data furnished by Dr. Collins' *Practical Treatise.*

TABLE XIV.

SHOWING THE MORTALITY OF CHILDBEARING WOMEN IN THE DUBLIN HOSPITAL DURING DR. COLLINS' MASTERSHIP.

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54	Total.
No. of Mothers . .	762	4862	5309	3817	1210	397	22	6	16,385
Deaths . . .	10	38	41	41	26	4	...	...	160
Percentage . . .	1·312	·781	·772	1·074	2·148	1·007	...	...	·976
Or 1 in every . .	76	128	129	93	46	99	...	...	102

It is not corrected for primiparity, and I bring it forward for its own value, and because it gives the data for periods of five years. It will be observed that its results agree in the main with those derived from the data of Dr. Farr. But a further step is attained by the five-year divisions showing that youthfulness is influential chiefly below 20 years, and increasing age not until 30 years are passed. These results are, in my opinion, however, somewhat modified by the data which follow.

The next table (XVI.) contains only multiparæ. Primiparæ are excluded; there is therefore no correction to be made. Further, it is not estimated; the figures all show actual values.[1]

It is worthy of remark that this table of actual values shows a minimum of mortality at the age of from 25 to 29 years.

The last table (XVII.) to be given is a composite one, but appears to me to be of value with reference to the present question. It is made up as follows:—

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[1] To complete the view of the mortality of childbearing women in Edinburgh and Glasgow, in 1855, I here give the mortality of the primiparæ. It is not placed in the text because of the smallness of the figures compared with those of multiparæ, and because the table of multiparæ is more like the others in the text.

TABLE XV.—SHOWING THE MORTALITY OF PRIMIPARÆ AT DIFFERENT AGES IN EDINBURGH AND GLASGOW IN 1855.

Ages . . .	15-19	20-24	25-29	30-34	35-39	40-44	45-49	Total.
No. of Primiparæ	331	1859	1007	354	134	33	4	3722
Deaths of ditto .	4	24	13	8	1	...	...	50
Percentage .	1·208	1·291	1·291	2·260	·746	...	...	1·343
Or 1 in every .	83	77	77	44	134	...	...	74

TABLE XVI.

SHOWING THE MORTALITY OF MULTIPARÆ AT DIFFERENT AGES IN EDINBURGH AND GLASGOW IN 1855.

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45-49	Total.
Multiparæ	45	1829	4030	3496	2273	807	92	12,572
Deaths in ditto . . .	...	13	21	39	20	8	2	103
Percentage	...	·711	·521	1·115	·879	·991	2·174	·819
Or 1 in every	...	140	192	89	113	100	46	122

TABLE XVII.

SHOWING THE COMPARATIVE MORTALITY OF CHILDBEARING WOMEN AT DIFFERENT AGES.

Ages	15-19	20-24	25-29	30-34	35-39	40-44	45-49	50-54
Mothers (Collins) . . .	762	4862	5309	3817	1210	397	22	6
Deaths, corrected for primiparity	10½	59	89	91½	52	15	1	
Percentage	1·3	1·2	1·6	2·3	4·3	3·7	4·5	
Or 1 in every	72	82	60	42	23	26	22	

With the number of women confined at different ages in the Dublin Hospital under Dr. Collins, are compared the deaths at different ages recorded by Drs. Collins, M'Clintock and Hardy, and Johnston and Sinclair. These deaths have been collected with considerable care, to insure a close approach to their true number. All the deaths were among women delivered in the same hospital, and these are compared with another set of cases also delivered in it. The deaths are corrected, as in former tables, for primiparity. The resulting percentages are of course of value only when compared with one another, and in this respect they appear to me to be very valuable. The smallest mortality is seen to be in the age 20-24 ; and the increased mortality from greater youthfulness is in the next quinquennial period less than the corresponding increase on the other side from greater age.

Looking over these tables, one cannot doubt that the result of Farr's data, showing 25 to 35 as the age of smallest mortality, may be considered as justly supplanted by the results of the tables showing quinquennial periods. In all of these the smallest mortality is found to be under 30 years of age. Of the tables showing quinquennial periods, given in the text, No. XVII. gives 20-24 as the safest age for parturition, while the XVIth gives it as 25 to 29 ; and we may guess with considerable assurance that the age of minimum mortality from parturition is at or near 25 years.

The following are the chief conclusions deducible from the whole exposition :—

1*st.* Youthfulness has less influence in producing mortality from parturition than elderliness.

2*d.* From the earliest stage of childbearing there is a climax of diminishing puerperal mortality,[1] succeeded by an anti-climax of puerperal mortality increasing till the end of childbearing life.

3*d.* The age of least mortality is near 25 years, and on either side of this age mortality gradually increases with the diminution or increase of age.

4*th.* Above 25 years puerperal mortality increases at a much higher rate than it increases at corresponding periods below 25 years, a circumstance which decidedly throws the greater safety to the side of the quinquenniad 20-25.

5*th.* Though it is not deducible from anything in this part, it is too interesting to escape notice that the age of greatest safety in parturition coincides with the age of greatest fecundity, and that during the whole of childbearing life safety in parturition appears to be directly as fecundity, and *vice versa.* "To the female sex," says Aristotle, "premature wedlock is peculiarly dangerous, since, in consequence of anticipating the demands of nature, many of them suffer greatly in childbirth, and many of them die."[2]

On the other hand, the practical obstetrician can feel no astonishment at the influence of age and multiparity, for he has only to reflect on the history of after-pains, phlebitis, phlegmasia dolens, rupture of uterus, and twin-bearing, in order to find sources of its explanation.

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[1] See Horwitz, *Monatsschrift für Geb.* etc., 1868, Bd. xxxii. S. 296.

[2] Sadler, *Law of Population*, vol. ii. p. 272.

CHAPTER VI.

THE DURATION OF LABOUR IN RELATION TO CHILDBED MORTALITY.

THE progress of obstetrics is not characterised, as is that of some of the more exact sciences, by a secure and gradual advance with unassailable step, always conquering some part of the region of the unknown. Our science, seeking to enlarge the boundaries of what is certain and fixed, makes its conquests from the unknown in a field, wide indeed, and surrounding it on every side, composed, in its nearer parts, of doctrines more or less nearly approaching in stability to those admitted within the true boundaries of the science, but, in its more distant regions, of mere shadowy hypotheses, that have not yet acquired any roots, and of ephemeral conjectures, often the offspring of shallowness, of special pleadings, and of vanity.

It is my object to claim a place in the science of Midwifery for the doctrine of the Duration of Labour —a doctrine exceeded in importance by none within the limits of obstetrics, and having the most extensive bearings upon that invaluable art or practice of the accoucheur of which the science is the chief expositor. It and similar doctrines have been deprived of their real features and importance, and hid from general appreciation, by the violent and not always seemly struggles which have taken place upon them, and which have uniformly ended, like many battles with more sanguinary weapons, either in absence of real result, or in the more or less complete discomfiture of all the contending parties. But perhaps the medical philosophers of another age will have wisdom to regard, without pity or shame, these squabbles of our day as necessary episodes in the story of the progress of imperfect beings towards perfect truth—in the progress of human intellects towards real science.

The doctrine of the duration of labour has been the real centre of many discussions which have been invested with other names, derived from some therapeutical principle which has been supposed to receive confirmation or confutation from its bearings upon it. In these discussions the obstetric schools of Edinburgh and of Dublin have more than once been found on opposite sides, as if truth were indicated by different symbols in the two countries. A dispassionate inquirer, perusing these interesting discussions, will not fail to discover that, while each party had much truth as well as error in its arguments, each, with a blind zeal, attacked indiscriminately both truth and error in its opponents.

The chief practical questions which have been investigated in connection with the doctrine of the duration of labour are the artificial dilatation of the os uteri, certain other points in the management of protracted labour, turning as a substitute for craniotomy, and the use of anæsthesia in midwifery. With these questions I shall not at present interfere; only I may cite them occasionally to illustrate and facilitate the development of the great doctrine of the duration of labour, which is now my object. The names of Harvey, Denman, Osborn, Breen, Hamilton, Burns, Murphy, Collins, Beatty, Simpson, Veit, and Busch, will always be honourably associated with the history of this doctrine. If in the sequel I do not frequently refer to all these writers, it is not because I lightly appreciate their labours, but because the subject appears to me to have now arrived at a stage at which it may with advantage be as far as possible dissociated from those various questions, which have been its parents, but would at present only injuriously encumber it. It is necessary to add that the two great names of Collins and Simpson are involved in the latest dispute on this subject. In its various stages much talent was shown, and much truth elicited on both sides. With this last discussion I am best acquainted, and will naturally, therefore, refer to it more than to the views of the other authors distinguished in connection with the subject.

Into the questions we shall have to discuss the use of statistics has been introduced; and it would be difficult to decide whether their application has tended more to elucidate or to confuse. It is evident that accurate statistics can never yield false results; but false results are easily made to appear as if flowing from them. In other words, if a disputant resorts to statistics, without the most careful use of logic, he easily flatters himself that they really supply the results he wishes from them. Against this fatal seduction into error many beacons have been erected, but they have not produced the safeguard anticipated by their sanguine authors. The present discussion, like many others in obstetrics, will afford clear examples of this abuse of a means of research which is among the most valuable on points where it is really available.

We have now to propound and prove two propositions.

1*st Proposition.*—The mortality of women in parturition and childbed increases with the increasing duration of labour (in an undetermined ratio).

2*d Proposition.*—The duration of labour is only an inconsiderable item among the many causes (single or combined) of the mortality of women in parturition and childbed.

These two propositions have hitherto been either confounded together, or made to conflict with one another. They really stand side by side, declaring separate truths, between which no collision can justly be made to arise.

1*st Proposition—The mortality of women in parturition and childbed increases with the increasing duration of labour* (*in an undetermined ratio*).

This proposition is one which easily gains credence, when the obstetrician reflects on the abstract nature of it. It is one whose practical bearings are most remote and indefinite. But although this is the case, it enunciates a solid truth, and can never be with justice either neglected or depreciated. The proposition does not affirm anything whatever in regard to the influence of prolongation of labour upon the consequent maternal mortality; nor does it affirm anything whatever as to the dangerousness of the pains of labour. It affirms nothing in regard to any individual case. It merely asserts the general law, that as labours increase in duration, or become protracted, they are also accompanied or followed by a greater maternal mortality.

A proposition such as this scarcely requires proof. As labour becomes protracted, so does life; and we know that every hour of life added in adult age increases the mortality of mankind. But in the human female many dangers accompany the function of childbearing, and combine to raise, for the childbed month at least, the mortality of females very far above what can be accounted for by the mere general law applicable to all mankind. The dangers of childbearing are, for the most part, concentrated into the period of labour, or derive from it their origin. The longer the labour, there will be the more opportunities for such dangers to intervene; and hence it naturally follows, that the mortality of women in parturition and childbed increases with the increasing duration of labour.

But this proposition has been confirmed by numerical investigations. I shall avail myself of Simpson's careful calculations,[1] made from the data contained in

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[1] *Provincial Med. and Surg. Journal*, 1848, p. 602.

Dr. Collins' admirable report of the Dublin Lying-in Hospital for a like purpose. Dr. Collins has in his report stated the duration of labour in 15,850 cases, of which 138 proved fatal. Table XVIII. exhibits these cases, arranged so as to show that the maternal mortality increases as the duration of the process of labour is augmented. It requires no explanation or commentary.

TABLE XVIII.

Duration of Labour.	Number of Deliveries.	Number of Deaths.	Proportion of Deaths.
Within 1 hour . .	3537	11	1 in 322
From 2 to 3 hours .	6000	26	1 in 231
From 4 to 6 hours .	3875	29	1 in 134
From 7 to 12 hours .	1672	21	1 in 80
From 13 to 24 hours	502	19	1 in 26
From 25 to 36 hours	134	8	1 in 17
Above 36 hours .	130	24	1 in 6

Such, then, is the statement and demonstration of this proposition.

It will be observed that the table of Dr. Collins' data gives us no information as to the special mortality of labours of extremely short duration, finished at various periods less than one hour. It is a very general opinion, and I believe a very correct one, that very rapid labours are, comparatively speaking, injurious and dangerous. And more minute investigation, as to the relations of very brief labours to maternal mortality, will probably show that there is a limit, at some point within an hour, beneath which, if labours go on diminishing in brevity, they increase in mortality.[1]

It must also be kept in mind that the peculiar case of primiparous women is included in the data. These have such peculiar conditions and dangers as must manifestly render their admixture with others prejudicial to the value of the data as demonstrating the proposition under consideration. It is desirable to have tables like that cited, composed of labours following pregnancies all of the same number.

It is not my purpose here to trace further than in a single author the history of this proposition. It has been stated, in terms almost identical with those I have used, by Professor Simpson, and confirmed by the table which I have adduced. To him, therefore, belongs the merit of formally enunciating it.[2] This we admit, although it would be scarcely a stretch of literary justice to refuse him any credit whatever in connection with it; for it will afterwards appear that he has so misunderstood and so used the principle, and the table on which alone he founds it, that his merit in the matter can be established only by separating the two or three sentences containing the bare principle and table from the mass of writing and argument in which he has enveloped them.

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[1] Hippocrates is quoted by Tarnier (*De la Fièvre Puerpérale*, p. 63) as saying that a sudden and easy labour should be looked on with suspicion, especially if the woman be already sick or languishing. Such labours (he adds) have often the most fatal consequences.

[2] *Provincial Med. and Surg. Journal*, loc. cit.; *Obstetric Works*, vol. i. p. 527.

We find this author first using the statistics of Table XVIII. to show that "the mortality accompanying labour is regulated principally by the previous length and degree of the patient's sufferings and struggles. In the Dublin Lying-in Hospital (he says), when under Dr. Collins' able care, out of all the women, 7050 in number, who were delivered within a period of two hours from the commencement of labour, twenty-two died, or one in every 320. In 452 of his cases, the labour was prolonged above twenty hours; and of these 452, forty-two died, or one in every eleven,—a difference enormous in its amount, and one surely calculated to force us all to think seriously and dispassionately of the effects of severe suffering upon the maternal constitution."[1] Now, it is evident that these statistics afford no ground whatever for such reflections. No doubt, sufferings and struggles are important elements in the history of any labour or set of labours; but nothing in regard to the influence of sufferings and struggles upon the mortality of parturition can be wrested from the statistics adduced. These statistics support only the general proposition (the first) as to the relation of duration to mortality of labour. This relation is determined by a thousand circumstances, known and unknown, besides sufferings and struggles, in regard to which last it affords scarcely the slightest presumption of any special baneful influence.

[1] *Monthly Journal of Medical Science*, October 1848; and *Obstetric Works*, vol. ii. p. 689.

When thus using Dr. Collins' data, Sir J. Y. Simpson was simultaneously engaged in his defence of anæsthesia in midwifery. In this cause, searching everywhere for arguments to convince Professor Meigs, he may be to a great extent excused, even when again falling into his former error in the use of these statistics. Addressing his transatlantic friend, and speaking of the pain of labour, he says, "It is safe in proportion to its shortness, and dangerous in proportion to its length. In the Dublin Hospital, the tables of which afford the only data on this point that I know to refer to, when the women were four hours in labour, more subsequently died than when their pain did not exceed two hours; of those that were eight hours in labour, more subsequently died than of those that were four hours ill; of those that were twelve hours in suffering, more died than of those that were eight: and so on, in a regular progression. The longer this supposed salutary and conservative manifestation of life-force (as Dr. Meigs terms it), the greater became the mortality. . . . etc."[1] It is not to be wondered at that this argument did not convince Dr. Meigs, since it is as illogical in its use as it is wrong in its essence. What accoucheur could for a moment resist the argument, if true? It is not our object here to discuss the influence of painfulness, or sufferings and struggles, or, in short, of whatever anæsthesia could annul, upon the maternal mortality of labours; we shall only say, that all accoucheurs must recognise it as a great exaggeration, to imply that pain, etc., has any such immense influence as Dr. Meigs is asked to believe. Were it so, then anæsthesia should deprive parturition of its most formidable sources of mortality.

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[1] *Association Medical Journal*, July 1853, p. 582. *Obstetric Works* vol. ii. p. 710.

In defending his views with regard to turning in cases of deformed pelvis, we find the same author reverting to the same statistics of Dr. Collins for assistance. Here he supplies evidence against his own former use of these data, or *vice versâ*. For he now interprets them as affording "ample evidence that, contrary to the general opinion of the obstetric profession, the mere length of the labour is a most serious and important element in reference to the degree of danger and fatality accompanying the process."[1] But again, it will be evident that these statistics afford no ground for attributing the maternal mortality to length or duration of labour as a cause, just as they afforded no ground for attributing the same mortality to the pain, etc., of the process. The proposition, that the increasing length of labour is accompanied by an increasing mortality, is a proposition at once true and proved by the statistics in question; while the proposition, that the "mere length of the labour is a most serious and important element in reference to the degree of danger and fatality accompanying the process," is one, to say the least, very questionable, and one to which the statistics afford no countenance. It is not necessary further to point out that, if the statistics so often referred to show that pain, etc., is the cause of the mortality, the same statistics cannot show that the mere duration is the cause of it; and if they prove either of these two points, they cannot be fairly extended so as to demonstrate our first proposition.

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[1] *Provincial Med. and Surg. Journal*, Feb. 9, 1848, p. 58; and *Obstetric Works*, vol. i. p. 527.

Dr. Collins justly objected to Dr. Simpson's uses of his data. The truth that was in them Collins rejected along with the error. A man of practical sagacity and immense experience, he at once repelled Dr. Simpson's erroneous conclusions, from the data in his *Practical Treatise*, in regard to the influence of pain and of length of labour upon maternal mortality. The inward testimony of his experience was so strong as to lead him instantly, and without analysing the statistical reasoning, to denounce these conclusions as visionary and extravagant. The truth of our first proposition he never grappled with. It had no apparent practical bearings; and therefore he refused to consider it.

Dr. Collins might have gone a little further. It would have been quite a legitimate use of Sir J. Y. Simpson's argument, as to the influence of length of labour upon the maternal mortality, to turn it against the whole practice of anæsthesia in midwifery. For it is a very general belief that anæsthetics, by diminishing the force of the uterine contractions, increase the duration of labour, at least in many cases. Hence it follows, if Dr. Simpson is right in regard to the baneful influence of mere length of labour, that anæsthesia must tend to increase the maternal mortality. But as we have shown that the statistics do not demonstrate this baneful influence of mere length of labour, the opponents of anæsthesia are deprived of this otherwise strong argument provided for them by the greatest promoter of the practice.

Before advancing to the second proposition, I shall illustrate the errors fallen into with regard to the first by a reference to a subject long within the recognised domain of statistics.

TABLE XIX.

Period of Life.	Proportion of Deaths.
At the age of 20 years	1 in every 141
„ „ 30 „	1 „ 99
„ „ 40 „	1 „ 77
„ „ 50 „	1 „ 74

This XIXth table may be assumed to be a correct statement of the mortality of mankind at different periods of life. An intelligent actuary will at once say that it proves that the mortality of mankind increases with the increasing duration of life, just as he would recognise our former table as bearing direct testimony to the truth of our first proposition. But such an actuary will never say or admit that the adjoining table proves anything with regard to the sufferings and struggles, or pain, endured by mankind, or in regard to the effects of advancing life. It cannot be proved by our former table that the mortality accompanying labour is regulated principally by the previous length and degree of the patient's sufferings and struggles (nor is it true); so it cannot be proved by this table that the mortality of mankind is regulated principally by the previous length and degree of the individual's sufferings and struggles (nor is it true). It cannot be proved by our former table that the sufferings of labour are safe in proportion to their shortness, and dangerous in proportion to their length (nor is it true); so it cannot be proved by this table that the pains occurring during life are safe in proportion to their shortness, and dangerous in proportion to their length (nor is it true). It cannot be proved by our former table that, contrary to the general opinion of the obstetric profession, the mere length of labour is a most serious and important element in reference to the degree of danger and fatality accompanying the process (nor is it true); so it cannot be proved by this table that, contrary to the general opinion of mankind, and of the medical profession, the mere length of life is a most serious and important element in reference to the degree of danger and fatality accompanying life (nor is it true).

2d Proposition—The duration of labour is only an inconsiderable part of the many causes (single or combined) of the mortality of women in parturition and the subsequent childbed.

As we have, under our first proposition, cleared away many of the incumbrances of the whole subject, the treatment of this second will be much more brief.

There is no obstetrical doctrine more deeply impressed on all the valuable literature of our profession than this, that the mere duration of labour, considered in itself and apart from other causes of danger likely to spring up as the process becomes protracted, is of little importance, so far, at least, as recovery of the mother is concerned.[1] This doctrine is embodied in the ever-recurring inculcation of patience, as the highest virtue of both mother and attendant, in many and various circumstances of distress during labour. Sometimes it is expressed in an apophthegm, " Meddlesome midwifery is bad;" at all times it is diligently instilled into the minds of young midwives and accoucheurs. Unlike our first proposition, a comparatively barren theorem, this is one of the best recognised and most valuable doctrines in obstetrics. It is, therefore, of the utmost consequence to defend and confirm it.

The proposition does not affirm that the mere duration of labour is of no importance,—quite the reverse. Far less does it affirm that the duration of labour, with the accompanying pain and struggles, is not a very considerable element in the history of every case. It says nothing in regard to the very important effects of the duration of labour after bad symptoms or dangerous complications have supervened. It asserts that the duration of labour is in itself (*per se*) only an inconsiderable part (probably a very inconsiderable part) of the many causes of the mortality of women from parturition and its consequences.

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[1] See Harvey's Works, Sydenham edition, p. 534, for his opinion of the influence of duration of labour.

Perhaps the strongest evidence in favour of this proposition is the fact, that it is the ancient and generally received opinion of the profession.[1] It rests upon what may be called the instincts of all experienced accoucheurs. In a science like medicine, where so little is capable of absolute demonstration, ancient traditions, especially if supported by the opinions of the great and wise, are among the most valuable and trustworthy guides of practice.

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[1] In attempting the defence of the opposite view, Sir J. Y. Simpson says—"I am fully aware that when I state my conviction, that the mere degree of duration and continuance of a labour is *per se* dangerous both to the mother and child, and very often fatal even in its influence, I venture to broach a doctrine which stands up alike against the opinion and the practice of some of the highest authorities in the obstetric profession.

"About half-a-century ago, when treating of the influence of the duration of labour in difficult and instrumental deliveries, Dr. Osborn observed—'I believe it is confirmed by general observation, that women recover at least as well after long, lingering, and laborious labours, the duration of which may have been extended to several days, as after the easiest, quickest, and most natural delivery.' In making this remark, Dr. Osborn stated, not his own opinion only, but, I believe, the general opinion of the accoucheurs of his time; and the same doctrine, little or not at all modified, still continues to be taught and acted upon, down to the present day, in the great English and Irish schools of midwifery, as the able and excellent writings of (for example) Professors Davis and Murphy, in London, and Drs. Collins and Beatty, in Dublin, etc., fully testify."—*Provincial Medical and Surgical Journal*, Feb. 9, 1848, p. 57.

In contrast with the above quotation, the student may do well to peruse some remarks by Dr. T. E. Beatty in his *Contributions to Medicine and Midwifery* (p. 43).

But the proposition may be supported most satisfactorily, both by direct and indirect evidence. Were it true that, "contrary to the general opinion of the obstetric profession, the mere length of the labour is a most serious and important element in reference to the degree of danger and fatality accompanying the process," then a well-established rule of philosophising must be declared to be at fault. It was a maxim of Newton's, that no more causes are to be admitted than are true and sufficient to explain the effects. Few indeed will ever be found to assert that any obstetric patient dies without a very evident, true, and sufficient cause. The causes of such deaths are very various no doubt; but the mere length of labour is, by Newton's maxim, excluded from the number, as the truth of its influence is in question, and it is not required to explain the phenomena.

Moreover, it is always true in nature that uniformity of cause insures uniformity of effect. This axiom also is at variance with the belief that mere duration of labour is an important cause of fatality in the process. For it is a common observation, that after long labours, even after the longest uncomplicated labours, there is often unusually rapid recovery. In the great mass of very long cases there is generally present some distinct and dangerous complication, which obscures the influence of the mere length of the labour, and destroys their value as arguments with regard to the effects of mere protraction. Again, in short and easy labours, where duration as a cause of fatality, supposed by some to be supremely important, is absent, there is still a considerable mortality.

Dr. Collins has distinguished himself by his zealous defence of the doctrine embodied in our second proposition, maintaining, as he does, that the mortality from protraction of labour, apart from other causes, is comparatively small. His elaborate *Practical Treatise* contains no record of any patient dying from the mere length of the labour; and his experience, founded on his wide field of observation, leads him to consider mere protraction of labour an inconsiderable cause of maternal mortality. It would be difficult to adduce statistics, at least from Dr. Collins' work to prove our second proposition. We have already shown how erroneously statistics framed from the data in his work have been used, and pushed forward as if proving that our second proposition is false. But some of Dr. Collins' data are almost as valuable as if they were positive proofs, from the light which they throw on the real causes of death in protracted cases.

To take one aspect of Dr. Collins' cases, as he has himself given it.[1] Of 16,414 parturient women under his care in the Dublin Lying-in Hospital, forty-two died whose labours were longer than twenty hours. "Of the forty-two, three died of typhus fever; nine of puerperal fever; one of stricture of the intestine, with effusion into the thorax; three where the placenta was retained; two of convulsions; one of abdominal inflammation previous to labour; nine of rupture of the uterus; one of inflammation of the intestines, with pus in the uterine sinuses; three of anomalous disease; one of diffuse cellular inflammation; six of inflammation, etc., subsequent to difficult labour; one of ulceration and sloughing of the vagina; one of disease of the lungs and hæmorrhage; and one of abdominal abscess." Here it is evident that we have a list of causes of death, apart from mere duration of labour, in all the cases where the length of the process exceeded twenty hours. No doubt the mere length of the labour may have been an aggravation in all these cases, but of this there is no evidence whatever in Dr. Collins' data, however arranged; and we must accept the opinion of Dr. Collins, who took care of all the cases—an opinion sanctioned by previous general acceptation for ages, that protraction of labour was an inconsiderable part of the many causes of this maternal mortality in childbed.

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[1] *Provincial Medical and Surgical Journal*, Oct. 18, 1848, p. 573.

The true bearing upon the great question before us, of the statement just quoted from Dr. Collins, has been altogether misconceived in some quarters. Dr. Collins' statement has been represented as "a list merely of such injuries and diseases as tedious labour does produce;" and it is added, as if it were an apt illustration, that "long ago surgeons always used to argue, in regard to their lithotomy and other cases, that the deaths were from inflammation of the bladder, or inflammation of the intestines, or disease of the kidneys, or of the liver, or—anything, in fact, but the operation itself. Modern surgery (it is said) does not admit of such pathological casuistry. Nor does modern midwifery."[1] It is scarcely worth while to stop to contradict the indiscreet reproach so easily cast upon old surgery and surgeons. Let us submit for a moment, and for argument's sake, to consider it true—and only for a moment, as its irrelevancy will be easily made apparent. These old surgeons argued that their patients did not die of lithotomy, or of its consequences. Dr. Collins does not argue that his patients did not die of labour and its consequences; on the contrary, he admits it. Dr. Collins argues, in opposition to Dr. Simpson, that the "mere length of labour was not a cause of death." To make a just use of the analogy above given, Dr. Simpson should have condemned the old surgeons for not considering the mere duration of the operation of lithotomy as a chief cause of the mortality of the operation. Dr. Simpson wishes us to condemn the old surgeons for not admitting inflammation of the bladder and intestines, etc., as causes of death in connection with lithotomy. In his zeal to prove the importance of mere duration of labour in reference to the fatality of the process, he censures Dr. Collins for admitting exactly analogous diseases as causes of death in connection with labour. Moreover, when Dr. Simpson speaks of "tedious" labour, he uses a well-known term, implying a great deal more than mere length of labour. When he says that tedious labour produces such diseases as Dr. Collins enumerates, then he and Dr. Collins are at one, and he had no right to address him as if committing a very great error. When he says that tedious labour produces these effects, he is not differing from, but agreeing with, the whole profession; only, he is deserting the position which Dr. Collins attacked, and which he would fain appear still to hold. For his statement is, not that tedious labour leads to these causes of death—a true one; but "that the mere degree of duration and continuance of labour is, *per se*, dangerous both to the mother and child, and very often fatal even in its influence;"—a doctrine without foundation.

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[1] *Provincial Medical and Surgical Journal,* Nov. 1, 1848, p. 506.

The element of mere duration of labour is, in fatal cases, so mixed up with other circumstances, that I despair of medical philosophers being ever able so to handle obstetric statistics as to make them yield anything like an approximation to a proper estimate of its baneful influence. In protracted cases, with no other evident dangerous complication, it is a common remark that the patients appear to make unusually rapid recoveries.

In tedious cases it is not the protraction which causes the complications and danger, but the complications which cause the protraction and danger, leaving the mere protraction as a negation destitute of any presiding influence.

Such is the statement of, and evidence for, our second proposition.

In the discussion between Dr. Collins and Dr. Simpson as to the influence of mere duration of labour upon maternal mortality, we have seen that the latter, by his use of Table XVIII., tried to prove that Dr. Collins was wrong in asserting that the mortality of mothers from protracted labour was strikingly small. Although Dr. Collins was not very happy in his statement of his views, and sometimes not to be justified in his arguments, yet there can be no doubt that the essence of the truth of our second proposition, as bearing on practice, was contained in his defence of his views.

Dr. Collins was personally engaged in watching and managing the great mass of cases reported in his valuable *Practical Treatise.* This circumstance will always give his views a peculiar force and value, even were his reputation as an author and observer not so high as it deservedly is. It was at least rash in any author, addressing Dr. Collins, to say—" Against the truth of your own recorded opinions I appeal to the truth of your own recorded facts. Against your own doctrines I appeal merely to your own data." Such are indeed very tame expressions, compared with others that appeared in this controversy. And yet we think we have made it evident that Collins, in common with the general mass of the profession, was right in regard to the main question, and his opponent wrong. Any one who reads the controversy will find in it an admirable illustration of the fable of the two knights looking at opposite sides of the same shield. But, although to a careful perusal this becomes evident, it is only just to add, that with Dr. Collins rested the practical truth, fairly founded on experience, while some theoretical truth was fitfully maintained by his opponent, yet so as almost to be concealed by error.

Let us consider for a moment what such reasoning as Sir J. Y. Simpson adopts in this controversy would lead to. It appears to us that, if he had looked whither his arguments might lead, he would have himself been probably deterred from urging them. If mere length of labour be an important element in the causation of deaths from labour, then certainly patience is no virtue in an accoucheur. If mere length of labour be as he describes it, then meddlesome midwifery must, I fear, be declared good instead of bad. If mere length of labour be as important as he represents it, then any treatment which will accelerate delivery may be easily defended. If it be right to disregard all the real causes of danger and death in labours, as this author does, in order to make prominent the danger of protraction, with the ulterior view of supporting an artificial interference which accelerates the process, then a like reasoning may be used to support the most absurd and unjustifiable measures, and the art of midwifery will be at the mercy of any specious reasoner, however ill founded his arguments may be.

It would be a waste of words to enter further on this discussion of the influence of the doctrine of the duration of labour. This doctrine has important relations to the mortality of children[1] in parturition, and to other matters. It is enough here to point out, that, in connection with these questions, the same errors in reasoning have been committed as have been made in tracing the bearing of the doctrine on the mortality of mothers.

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[1] I beg to refer the student who wishes to pursue this subject to a paper by Gassner in the *Monatssch. für Geb.* 1862

CHAPTER VII.

ON AGGREGATION AS A SOURCE OF DANGER TO LYING-IN WOMEN.

It has been of late frequently asserted that the mere congregation of human beings in towns is the cause of much shortening or loss of life, and that this loss of life or danger to life is greater as the density of the population is increased. It has also been affirmed, and if the foregoing assertion be true, it follows as a matter of course, that the mere aggregation of human beings in one building is a source of danger to their lives. This latter danger, again, will of course be increased if the human beings are sick, and also be in proportion to the degree of their crowding, or to the smallness of space occupied.

These allegations have, one or other, the support of such authorities as Stark[1] and Simpson,[2] of whom the latter leans upon Farr, Duncan, and Gairdner, but I do not inquire whether these latter gentlemen are justly used as props or not.

In regard to all of these assertions, I unhesitatingly express my assurance, that they are without any sufficient foundation. They form a climax rising from rural districts to towns, from open to crowded parts of towns, from towns to buildings, from buildings full of healthy to buildings full of sick human beings. If any of these assertions be true, or capable of proof, it should be that regarding the mortality of hospitals or buildings full of sick. This forms the head and front, the climax, of the alleged offence against good sanitation. In this chapter I consider this point, and I leave the reader who peruses it to judge whether or not the assertion is proved in regard to the kind of building and the individual building best adapted to test it.

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[1] *Edinburgh Medical Journal*, 1869, pp. 481, etc.

[2] *Lancet*, Nov. 1869, p. 700.

There can be no doubt that, as you leave the rural districts and pass through different degrees of aggregation on to overcrowding in an hospital, you have increasing and new sources of insalubrity. But these causes of insalubrity may be counteracted. They are not inevitable nor invincible. The highest authorities maintain, that a barrack may be made as salubrious as any residence. So may an hospital, for aught I can see. Authors have yet to prove that residence in well-arranged towns and hospitals is insalubrious. They have yet to prove that mere aggregation is, *per se*, pernicious in its influence. If such be the case, then no measures can avert the evils of town or of hospital residence. For my part, I believe, that even now we know how to make a town or an hospital as healthy as any other place, if the condition of overcrowding is prevented.

Authors who rashly make assertions such as those just given from Stark and Simpson, appear to me to rely on very insufficient evidence, to have a great power of closing the eye against most pertinent facts, or to have grievously neglected to show their readers how they evade the evidence of such facts. For instance, when they assert that towns are more unhealthy than rural districts, they are satisfied with giving us the mortality in the two kinds of places as evidence of their comparative healthiness. But it is plain, that such facts are mere statements of mortality, not evidence as to salubrity or insalubrity. Are the shambles insalubrious because every bullock there dies ? Are the rural districts around Metz insalubrious in 1870 because there is a great mortality there ? Is no distinction to be made between the insalubrity of Sheffield and the insalubrity of steel-grinding ? Is the ploughman or field-labourer in the country more or less healthy than the artisan or labourer in town employed in an equally healthy occupation ? I shall go no further in rebutting the assertions of Stark and Simpson : first, because there is probably some little truth in their views ; second, because the subject is foreign to my purpose ; and, third, because I have said enough to show where their arguments are utterly insecure.

Again it is asserted that the greater the aggregation the greater the insalubrity. Curiously enough, those who make this statement forget the gigantic fact of London—the largest aggregation of human beings that perhaps ever existed ; yet a town often said to be the healthiest in the kingdom, and which certainly has a smaller annual mortality than most large towns, and than many villages.

I have no doubt that, even now, towns are built in which the mortality is as small as in rural districts; barracks in which the mortality is as small as in towns; hospitals in which the mortality is as small as in the homes of the poor. I wish I could also say, that such is generally, or more than exceptionally, the case.

I propose now to inquire what information can be derived from the statistics of the Lying-in Hospital of Dublin, as to the influence of aggregation of lying-in women on their mortality. In the records of that institution there have been accumulating, for above 100 years, facts bearing upon this point. These facts are embodied in a table, showing the number annually delivered, and the corresponding yearly mortality. This table may be got in various places. I shall use that which is to be found at p. 30 of Dr. Evory Kennedy's little book, entitled *Hospitalism and Zymotic Diseases*, etc.

I suppose no one doubts that overcrowding the wards of a maternity, or of any other hospital, is one of the most certain causes of danger and death to the inmates. Every one has heard of the Black Hole of Calcutta, or of the Hôtel Dieu as a maternity in the last century. On this great subject I do not now propose to say a single word. I mean to show what information can be derived, with certainty, from the records of the Dublin Hospital, as to the influence on mortality resulting from the bringing together of different numbers of lying-in women. In other words, does the experience of the Dublin Hospital show that there has been, in bringing together for lying-in a small number, greater mortality than in bringing together a large number for the same purpose?

This inquiry will be valuable with a view to throwing light on the present discussion regarding hospitals. The information derived will, for reasons already given elsewhere, be more reliable for this purpose than that obtained by studying corresponding amputation statistics. Further, the information derived will be, in my opinion, valuable, so far as it goes, to a degree which no other statistical information can approach, because of the long time and the large numbers involved in this great hospital's experience—above 190,000 cases in 113 years. These data I here give in a tabular form (Table XX.)

These data have, as might be expected, been already used by students of the influence of aggregation. Especially, Dr. Evory Kennedy has availed himself of them; and, trusting to them almost alone, he tries to establish the proposition, "that the generation and absorption of this contagion (of puerperal metria) is in a direct proportion to the number of parturient females cohabiting in a given number of feet of atmospheric space at their parturient period, or who breathe the same atmosphere when lying-in."[1] So important does Dr. Kennedy consider this proposition, that he calls it his Redan proposition; and it truly is so. If it is secure, his cause is gained, and *vice versâ*.

Dr. Kennedy has made no proper analysis of the

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[1] *Hospitalism*, etc., p. 25.

TABLE XX.—SHOWING DEATH-RATE PER THOUSAND OF PATIENTS IN DUBLIN LYING-IN HOSPITAL FOR THE YEARS 1757 TO 1868 INCLUSIVE.

Year.	Patients.	Deaths.	Per 1000.	Year.	Patients.	Deaths.	Per 1000.	Year.	Patients.	Deaths.	Per 1000.	Year.	Patients.	Deaths.	Per 1000.
1757	55	1	18·1	1785	1292	8	6·2	1813	2484	62	24·9	1841	2025	23	11·4
1758	454	8	17·6	1786	1351	8	5·9	1814	2508	25	9·9	1842	2171	21	9·7
1759	406	5	12·3	1787	1347	10	7·5	1815	3075	17	5·5	1843	2188	22	10·1
1760	556	4	7·2	1788	1469	23	5·7	1816	3276	18	5·5	1844	2176	14	6·4
1761	521	9	17·3	1789	1435	25	17·4	1817	3473	32	9·2	1845	1411	35	24·8
1762	533	6	11·3	1790	1546	12	7·8	1818	3539	56	15·8	1846	2025	17	8·4
1763	488	9	18·4	1791	1602	25	15·6	1819	3197	94	29·4	1847	1703	47	27·6
1764	588	12	20·4	1792	1631	10	6·1	1820	2458	70	28·5	1848	1816	35	19·3
1765	533	6	11·3	1793	1757	19	10·8	1821	2849	22	7.7	1849	2063	38	18·4
1766	681	3	4·4	1794	1543	20	12·9	1822	2675	12	4·1	1850	1980	15	7·6
1767	664	11	16·6	1795	1503	7	4·7	1823	2584	59	22·8	1851	2070	14	6·8
1768	655	16	24·4	1796	1621	10	6·2	1824	2446	20	8·2	1852	1963	11	5·6
1769	642	8	12·5	1797	1712	13	7·6	1825	2746	26	9·5	1853	1906	17	8·9
1770	670	8	11·9	1798	1604	8	4·9	1826	2440	81	33·2	1854	1943	37	19·0
1771	695	5	7·2	1799	1537	10	6·5	1827	2550	33	12·9	1855	1060	35	33·0
1772	704	4	5·7	1800	1337	18	13·5	1828	2856	43	15·1	1856	1600	25	15·6
1773	694	13	18·7	1801	1725	30	17·4	1829	2141	34	15·9	1857	1509	33	21·9
1774	681	21	30·8	1802	1985	26	13·1	1830	2288	12	5·2	1858	1084	30	27·7
1775	728	5	6·9	1803	2028	44	21·7	1831	2176	12	5·5	1859	1389	21	15·1
1776	802	7	8·7	1804	1915	16	8·4	1832	2242	12	5·4	1860	1404	26	18·5
1777	835	7	8·4	1805	2220	12	5·4	1833	2138	12	5·6	1861	1135	59	51·9
1778	927	10	10·8	1806	2406	23	9·6	1834	2024	34	16·8	1862	800	58	72·5
1779	1011	8	7·9	1807	2511	12	4·8	1835	1902	34	17·8	1863	1228	32	26·1
1780	919	5	5·4	1808	2665	13	4·9	1836	1810	36	19·9	1864	1184	26	21·9
1781	1027	6	5·8	1809	2889	21	7·3	1837	1833	24	13·1	1865	1332	30	22·5
1782	990	6	6·1	1810	2854	29	10·2	1838	2126	45	21·2	1866	1074	40	37·2
1783	1167	15	12·8	1811	2561	24	9·4	1839	1951	25	12·8	1867	1146	40	34·9
1784	1261	11	8·7	1812	2766	43	15·6	1840	1521	26	17·1	1868	1022	39	38·2

Table on which he founds his proposition, but trusts to what he calls a bird's-eye view of it, and to a running commentary on its figures.[1] Both of these processes of taking the evidence derivable from the table are, to me at least, quite novel. To them Dr. Grimshaw adds another "upon the system of curvilinear test."[2] All three methods prove nothing whatever—show nothing towards proof of the proposition. They are not calculated, as they stand, to show anything. There is only one way of extracting the evidence from the table, and Dr. Kennedy has not tried it. Dr. Kennedy presents nothing in the way of proof of his proposition. He makes many statements bearing upon it, which different physicians will value differently, but he gives no positive proof.

Criticising Dr. Kennedy's grand proposition, Dr. Denham, Dr. Atthill, and Dr. M'Clintock[3] show, that so far from the data lending it any support, they at least appear to be hostile to it. While Dr. Kennedy gives no statistical evidence in proof of his proposition, they adduce abundant evidence to show that it is in the highest degree improbable. For instance, Dr. Atthill produces the following

TABLE XXI.

Years.		Deliveries.	Deaths.	Rates.
1800 to 1804	inclusive	8,990	134	1 in 67
1805 to 1809	„	12,691	81	1 in 157
1864 to 1868	„	5,758	175	1 in 31

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[1] *Hospitalism*, p. 118.

[2] *Ibid.* p. 123.

[3] *Dublin Quarterly Journal of Medical Science*, August 1869, pp. 276, 262, and 232.

The mortality in these periods of the hospital's history is in a sort of inverse proportion to the deliveries. This table does not disprove Dr. Kennedy's proposition. It renders it highly probable that Kennedy's proposition is opposed to truth. In favour of his own proposition, Dr. Kennedy adduces no evidence nearly so strong as this of Dr. Atthill against it.

But the Dublin Hospital data can, without difficulty or strain, be made to yield direct and exact evidence on the influence of aggregation upon its inmates; evidence, as already said, of the highest value, from the unity and other characters of delivery, from the length of the hospital's existence, and the consequent greatness of its figures. It is easy to avoid a misleading bird's-eye view, or a dangerous selection of special strongly-marked years that may favour a preconceived notion. The whole data can easily be interrogated, and the answer taken for every degree of aggregation. That answer will settle the question, so far as these invaluable hospital statistics can settle it, and the result will be certain and irrefragable. To obtain the solution of this question, then, I have arranged and tabulated the data as under :—

TABLE XXII.

SHOWING RATE OF MORTALITY PER THOUSAND AMONGST THE INMATES OF THE DUBLIN LYING-IN HOSPITAL FOR THE YEARS FROM 1757 TO 1868 INCLUSIVE, ARRANGED ACCORDING TO THE NUMBER OF HUNDREDS OF INMATES.

Women delivered.	Rate of Mortality per Thousand.			Number of Years or Observations.	Mean Age of Hospital.
	Lowest.	Average.	Highest.		
Less than 100 . .	18·1	18·1	18·1	1	1
400 and under 500	12·3	16·1	18·4	3	4
500 ,, 600	7·2	13·5	20·4	5	6
600 ,, 700	4·4	15·8	30·8	8	14
700 ,, 800	5·7	6·3	6·9	2	17
800 ,, 900	8·4	29·8	72·5	3	49
900 ,, 1000	5·4	7·4	10·8	3	24
1000 ,, 1100	5·8	25·0	38·2	6	78
1100 ,, 1200	12·8	30·4	51·9	4	88
1200 ,, 1300	6·2	13·7	26·1	3	55
1300 ,, 1400	5·9	12·9	22·5	5	63
1400 ,, 1500	15·7	19·1	24·8	4	64
1500 ,, 1600	4·7	11·8	21·9	6	56
1600 ,, 1700	4·9	9·7	15·6	5	51
1700 ,, 1800	7·6	15·8	27·6	4	53
1800 ,, 1900	13·1	17·4	19·9	3	84
1900 ,, 2000	5·6	11·6	19·0	8	80
2000 ,, 2100	6·8	13·9	21·7	6	81
2100 ,, 2200	5·5	10·6	21·2	7	81
2200 ,, 2300	5·2	5·3	5·4	3	66
2300 ,, 2400	—	—	—	—	—
2400 ,, 2500	8·2	20·9	33·2	5	62
2500 ,, 2600	4·8	20·0	22·8	5	60
2600 ,, 2700	4·1	4·5	4·9	2	59
2700 ,, 2800	9·5	12·5	15·6	2	62
2800 ,, 2900	7·3	11·0	15·1	4	61
2900 ,, 3000	—	—	—	—	—
3000 ,, 3100	5·5	5·5	5·5	1	59
3100 ,, 3200	29·4	29·4	29·4	1	63
3200 ,, 3300	5·5	5·5	5·5	1	60
3300 ,, 3400	—	—	—	—	—
3400 ,, 3500	9·2	9·2	9·2	1	61
3500 ,, 3600	15·8	15·8	15·8	1	62

Any one can read off for himself the results of this table. It is well worthy of study; and to facilitate the observation of its general conclusions, I give another condensed view of its facts.

TABLE XXIII.

SHOWING THE RATE OF MORTALITY PER THOUSAND AMONGST THE INMATES OF THE DUBLIN LYING-IN HOSPITAL, ARRANGED ACCORDING TO THE NUMBERS OF INMATES.

Women delivered.	Rate of Mortality per Thousand.			Number of Years or Observations.	Mean Age of Hospital.
	Lowest.	Average.	Highest.		
Less than 800 .	9·5	14·0	18·9	19	8
800 and under 1300	7·7	21·3	39·9	19	59
1300 „ 1800	7·8	13·9	22·5	24	57
1800 „ 2300	7·2	11·8	17·4	27	78
2300 „ 2800	6·6	14·5	19·1	14	61
2800 „ 3300	11·9	12·8	13·9	7	61
3300 and upwards .	12·5	12·5	12·5	2	61

Taking, then, the best data which, so far as I know, the world affords, as our sole, and as, in the meantime, a sufficient guide in this matter, we find that *the mortality of the Dublin Lying-in Hospital does not increase with the increased number of the inmates—does not rise with the aggregation. The mortality of the Dublin Lying-in Hospital is neither in the direct nor in the inverse ratio of the aggregation.*

The figures, indeed, seem to favour the view that the hospital is a better and safer institution the greater the aggregation. Certainly, a smaller proportional number die when there are many in it than when there are fewer. It is plain that we cannot look to aggregation as an important cause of mortality in the Dublin Hospital. This is a great practical result; for it sets inquiry into other directions to find out the hidden sources of increased mortality. Dr. Kennedy's proposition, above quoted, is not only not proved by the data he refers to—it is proved to be false. The opposite of his proposition is shown to be nearer the truth. With the fall of his Redan proposition, fall all his conclusions regarding puerperal fever and the advantages of small hospitals.

Although the supposed paramount evil influence of aggregation in this great hospital is now disproved, much more requires to be done with a view to discover and avert the causes of so-called metria, which is the chief source of variations in its death-rate.

Holding in view the splendid results secured by the antiseptic system of Professor Lister, the prospects of still further benefits which the study of the system opens up before the eyes of the surgical and obstetrical philosopher, and the corroborative evidence of Saxtorph in favour of its efficiency in preventing pyæmia, we cannot but be sanguine in our expectations that, as Syme predicted, a ready way of preventing insalubrity of hospital wards may be very soon established.

CHAPTER VIII.

ON PUERPERAL FEVER IN HOSPITAL AND IN PRIVATE PRACTICE.

To attempt to settle any disputed point, such as the the salubrity of hospitals, by using the mortality from metria or puerperal fever as the criterion, is a most unsatisfactory kind of proceeding, and certain to lead to no exact or convincing results. This arises simply from the disease being one regarding which very little is exactly known or even agreed upon, and the consequent uncertainty of the diagnosis or opinion of any physician regarding it, and the consequent still greater uncertainty that any two or more physicians alluding to it are giving figures or stating opinions regarding the same things. Dr. Evory Kennedy defines metria as including "puerperal fever, metritis, peritonitis, pleuritis, phlebitis, arthritis, pyæmia, purpuric or cerebro-spinal metria, traumatic metria, erysipelas, and hospital gangrene."[1] Although this appears to me to be the opposite of defining or settling, I shall not here enter upon any detailed objection to it, because it would involve a discussion not essential to my present purpose.

I have elsewhere[2] stated my reasons for adopting the total, and not merely the metria or puerperal-fever mortality, as the best criterion for judging of the salubrity of hospitals, yet as affording results that require to be corrected according to the varying conditions of the patients other than such as affect the salubrity of residence. But though this is the case, some good may come of discussing the metria mortality, taking up the subject as vulgarly treated in all modern writings, even though the ground is insecure and unworthy of confidence.

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[1] *Hospitalism*, p. 24.

[2] *Edin. Med. Jour.* Nov. 1869 ; and page 4 of this book.

Puerperal fever, or metria, is, to me, a hotbed of insufficient and false hypotheses. I do not believe there is any such single disease. The term includes a variety of diseases, and a variety of modifications or terminations of diseases. It is familiarly described as a zymotic disease. It is described universally as occurring in epidemics; not merely as an endemic. I feel certain, and believe I can prove, that an epidemic of puerperal fever never occurred—that no accepted definition of an epidemic can be made to include metria. When authors speak of epidemics of metria, of puerperal fever sweeping over a country, as they do speak, they are merely giving the reins to a misleading hypothesis which suits their present purposes or fancies. This disease is described as being contagious, or infectious, or both; but the authors who do this forget, with an almost ridiculous carelessness, to define what the disease is of which they are speaking so confidently, to show whether there is any such fever or not, and still more to define contagion and infection. I cannot refrain from referring to a common occurrence in medical and in general circles, as illustrating the present unsatisfactory state of the theory of this great disease, so called. When, in any town or country-side, even a single woman of position dies of puerperal fever, one immediately hears of childbed fever raging—of an epidemic.* If two or three cases occur simultaneously in the same exalted class, the outcry is dreadful about the pestilence. The doctors and the public are all talking. A superstitious terror, the natural child of ignorance, prevails. But though many die in the hovels of the poor or the cottages of a better sort, there is little talk among the doctors and less among the people. Both doctors and people seem ignorant of the fact that there is a regular and practically constant mortality from puerperal fever all around them.

In order to make arguments from the prevalence of metria tell against hospitals, it has been called preventable. The whole of Dr. Kennedy's essay on *Hospitalism* is based on this allegation. It may be true, but there is not a particle of evidence of its truth. The disease may have its ravages diminished, but it has yet to be shown that it can be altogether prevented. The term "preventable," so attached to metria, is a sufficient proof of the thoroughly unpractical or sensational character of the speculations of any writer who uses it as implying that we have means of preventing its appearance. It is, in truth, as little preventable as any disease in the nosology, or any crime in the statute-book. It is, possibly, preventable; but it has certainly never been prevented. It is preventable, and physicians are at hand; but it is common everywhere! It is preventable; but it occurs in spite of the combination of every circumstance that is known or supposed to prevent it!

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* See remarks at page 7.

Metria and pyæmia (certainly not very far from being identical) are the chief causes or indices of variations of mortality in hospital and in private practice. They are more frequent the worse the general health, the more depraved the constitutions of the patients. They are more frequent the worse or the more serious the kind of cases treated. They are also more frequent the worse the arrangements affecting the salubrity of the patient's bed and house. Positive statements like these respecting puerperal fever and pyæmia are not of a nature to be disputed by any one, and I believe they may be as nearly proved as is admitted by the nature of the case.

It has, during recent discussions regarding hospitals, been repeatedly asserted that puerperal fever is a constantly-existing or constantly-reappearing disease in them, or at least in the larger examples of them; and that it is only an occasionally and rarely-occurring disease in private practice. These statements are so important as to appear to me to demand very careful consideration and investigation. If they are true, they at least suggest a very powerful argument against even the best hospitals as at present constructed and managed. This is one source of the importance of the statements; but there are several others.

Speaking of this disease, Dr. Kennedy says—"Its local cause approaches more nearly to a *constant* quantity in the wards of a crowded lying-in hospital; whereas it is only an *occasional* quantity in the houses of the affluent."[1] Again, he says—"That in lying-in hospitals, where large numbers of patients are delivered under the same roof, the disease finds its habitat, appearing and reappearing at uncertain intervals."[2] Again, he adds—"It is therefore not a disease found to prevail in small lying-in hospitals, or cottages where only one or two patients cohabit in their lying-in."[3]

The first question to which I would direct special attention regards the alleged only occasional occurrence of this so-called puerperal fever outside of large hospitals. It is well known to be a common disease in large hospitals, and to have regularly ascribed to it a certain proportion of the hospital yearly mortality. Dr. Kennedy and others assert that this kind of statement is true only of large hospitals. Is it so?

With a view to getting the solution of this question, I might look to my own private practice, but there I find the numbers too small for comparison with large hospital yearly statistics. If with my private practice I include my consultation practice, then I find a regularly reappearing yearly quantum of puerperal fever, just as in hospitals. But I prefer, with a view to the greatest attainable accuracy, to take the public documents of the Registrar-General of Scotland. From these I have framed the following table, showing the reported mortality from metria in the towns of this part of the United Kingdom.

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[1] *Hospitalism*, etc., p. 10.
[2] *Ibid.* p. 25.
[3] *Ibid.* p. 26.

TABLE XXIV.

Year.	No. of Births.	No. of Deaths from Metria.	Proportion of Deaths from Metria to Births.
1860	42,158	116	1 in 363
1861	44,319	94	1 in 471
1862	44,054	59	1 in 746
1863	45,783	78	1 in 587
1864	47,321	114	1 in 415

This table affords a sufficient answer to the question raised. It shows that, out of hospitals, there is a regularly and constantly recurring quantum of mortality from puerperal fever or metria. Nothing more is required to show that, in this respect, hospital and private practice are alike, and that the assertions of Dr. Kennedy are erroneous. The table might have easily been extended, and with the same result, to the mainland-rural and to the insular districts of Scotland. No doubt the result is a truth comprehending the whole world.[1]

[1] I say nothing here as to the value of this table as an index of the actual amount of metria occurring in different years in the large towns of Scotland. To aid in forming a judgment on this point, I refer the reader to remarks already made at page 12. To contrast with this town mortality from metria (*quantum valeat*), I give the following data from the *Dublin Quarterly*, p. 250 :—" The death-rate

I shall now proceed a step further, and try to show whether or not hospitals and private practice are alike in the fraction of their gross mortality, or mortality from all causes, after parturition attributable to metria alone. The deaths in connection with parturition have been by many divided into three classes:—*First,* deaths from childbirth; *second,* deaths from puerperal fever or metria—that is, from causes connected with parturition other than are included under the head of childbirth deaths; *third,* deaths during or after parturition from causes quite unconnected, or supposed to be quite unconnected, with childbirth. The question now raised concerns the second of these categories. It is—Are hospitals, in contrast with private practice, remarkable for the high proportional share of metria in producing the gross mortality after parturition?

This is a very important question; for the answer given to it in different cases will form a very good criterion of the value of hospital or of private practice. Were puerperal fever well defined, it might form the best criterion; for an hospital cannot, of course, be blamed for its childbirth deaths, or mortality in the first category—that is, they are not the result of hospital arrangements, whether salubrious or not. As Messrs. Bristowe and Holmes say of deaths generally, and with truth so far as a fairly-arranged hospital is

from puerperal fever given by Dr. Collins is 1 in 297; by Dr. M'Clintock, 1 in 214; by Dr. Johnston, 1 in 289; and by Dr. E. Kennedy, 1 in 112." I do not enter on the consideration of these actual values, as I wish only to consider comparative values.

concerned, it may be said with certainty of childbirth deaths that the more of them the more is the efficiency and utility of the hospital demonstrated. But it is otherwise with metria alone regarded as a cause of death. It will increase with the gravity of the childbirth cases; but at the same time its amount will, no doubt, rise and fall with the salubrity of the same or of different hospitals.

To show the proportion of metria deaths in hospital and in private practice comparatively to the total deaths, I have constructed the following table:—

TABLE XXV.

Nature of Data.[1]	Total Mortality.	Metria Mortality.	Proportion of Metria to total Mortality.
Towns of Scotland, 1860 to 1864 inclusive	1254	461	1 in 2·7[2]
Private practice (M'Clintock)	131	52	1 in 2·5
Private practice (Beatty) .	30	17	1 in 1·8
Coombe Hospital, Dublin .	89	55	1 in 1·6
Rotunda, Dublin (Collins) .	164	56	1 in 3
Ditto (M'Clintock and Hardy)	65	31	1 in 2
Ditto (Johnston and Sinclair)	67	41	1 in 1·6
Ditto (E. Kennedy) . . .	224	171	1 in 1·3
Hospital practice (Beatty) .	17	11	1 in 1·5
Hospital practice (Churchill)	4	2	1 in 2
Montreal Hospital . . .	17	5	1 in 3·4
Waterford Hospital . . .	15	5	1 in 3

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[1] With the exception of those in the first line, all the data in this table are to be found in the report of the discussion in the Obstetrical Society of Dublin. See *Dublin Quarterly Medical Journal*, August 1869.

[2] Corrected to 1 in 3·6.

In the foregoing table the first line requires correction to make it justly comparable with the other data. This correction consists in adding to the total mortality there given (which is, in that line only, the mortality *of* childbed) the mortality from causes unconnected with childbirth (so as to make it then the mortality *in*, not *of*, childbed, as it is in the subsequent lines). This correction consists in adding to the mortality a third of the sums stated, the omitted deaths *in* (not *of*) childbed being, according to Dr. M'Clintock, a fourth of the total deaths *in* childbed, or a third of the deaths *of* childbed.

Now the table shows no very great amount of difference in the examples cited as to the proportion of the total deaths caused by metria in hospital and in private practice respectively.

In private practice, metria destroys 1 in 3·6, 1 in 2·5, 1 in 1·8 of those that die in childbed.

In hospital practice, metria destroys from 1 in 3·4 to 1 in 1·3 of those that die in childbed.

It is thus apparent, so far as the examples cited go, that metria or puerperal fever causes nearly as much havoc in private practice as in hospitals—at least in those that are pretty well arranged and managed.

There can be no doubt that in bad lying-in hospitals the proportion of the mortality due to metria will be far higher than any of the figures I have cited indicate. Bad arrangements will increase the proportionate quantity of metria mortality, whether in hospital or in private practice. The hospitals I have adduced will be universally admitted to be fair examples of their class, and will not be credited with the possession of a degree of perfection that may not be successfully imitated.

I have thus shown that puerperal mortality is a constant quantity in every kind of obstetric practice, and that it is only to a small degree more prevalent in certain hospitals than in private practice. This greater prevalence may be easily accounted for by reference to the general degradation, to the comparatively great number of the seduced, and to other unfavourable conditions, among those delivered in hospitals. There is no need to look for any cause of the greater mortality other than is active in rural or cottage practice.

In a previous chapter on the Mortality of Childbed, I have shown that there is not demonstrated so great a difference between the mortalities of childbed in fairly managed hospitals and in private practice as has been often, indeed generally, alleged; and I have expressed my opinion that the difference is easily accounted for, without attaching to hospitals and to their benevolent supporters those fearful charges of wholesale murder which recent writings imply. To this one might retort that I have here proved that metria—which, I admit, is fostered by bad arrangements—is a cause of more deaths in hospital than in private practice. While I admit that this is an apparent result of my demonstration, I must, in conclusion, show that it is not a real or justly reached result.

The difference between hospital and private practice in respect of the proportion of deaths attributable to metria is shown to be not great. The difference in favour of private practice is observed only in the towns of Scotland collectively. To show now the untenable character of the apparent result referred to, I shall merely point out that the towns of Scotland include a vast mass of happy, healthy, and comfortable women; while the hospital population consists of a vast mass of degraded, wretched, and often unhealthy women. To make a just comparison between hospitals and the towns of Scotland, we ought to place in contrast with hospitals, the data regarding that class of the poor who are like those that enter hospitals. Looking at the figures in the table, and also considering the results of the private practice of the justly eminent individuals there cited, one can scarcely doubt that, were they properly tried, hospitals would be at least not far from equal to private practice so far as regards lowness of death-rate.

I can find no ground for the awful suspicion that well-managed hospitals have caused a large, unnecessary, or avoidable mortality, or developed diseases previously unheard of.

CHAPTER IX.

ON THE QUESTION OF MATERNITY HOSPITALS.

To be, or not to be ?—that is the question. It is now supposed by many to be settled that they ought not to be. If I read current literature aright, the prevalent opinion is that, while medical and surgical hospitals are now on their trial, maternity hospitals are already condemned. The mortality of maternity hospitals is said to be so great that it is expedient, indeed absolutely necessary, to close them entirely. My object, in this brief chapter, is to lay before my professional brethren some results of my study of this subject.

The Dublin accoucheurs have always enjoyed a very high reputation, and I observe, from the lengthy reports of the Obstetrical Society of that city, that they are almost unanimous in resisting the popular opinion regarding maternity hospitals. They wish their great lying-in hospital improved, not destroyed. They lament the frequent prevalence in it of puerperal fever, which they wish new and additional means to prevent. They consider a well-managed maternity hospital to be, on the whole, a blessing, not a curse, to the class of women who resort there in their time of need.

The history of medicine, while it records no such sustained and able attack on hospitals as is now going on, is full of evidence of the value of these institutions. They have been, and continue to be, the nursing mothers of the science and art of medicine in all its branches. Without them little progress could have been made in the past; without them little hope of progress can be entertained for the future. Meantime, it is impossible to conceive the carrying on of sound medical education without them; and if this cannot be done, their destruction must inevitably be a great curse to the community. While there can be no doubt of their incalculable value to the public, it is said that they are a curse to their own inmates. My object is to inquire whether or not this charge against them is true, so far as maternity hospitals, which are said to be the worst, are concerned.

I enter on the subject with great care, for I regard the institutions attacked with the reverence due from a child to its mother; and I do not hesitate, in advance, to say that I regard the attack on hospitals, apart from its scientific merits or demerits, as conducted in a very indecent and injudicious manner. Hospitals demand the confidence and support of the public, and it surely might have been expected of medical men that they should not condemn and slander them until after a full discussion of their merits, and the attainment of a general agreement as to their beneficence or maleficence. Instead of this, medical men have been found unwise enough to slander them prematurely.

A method of condemning maternity hospitals, frequently used, is to quote from the work of Le Fort, *Des Maternités.* The following is a sample :—

> "Of 888,302 delivered in hospitals, 30,394 died, or 1 in 29;
> Of 934,781 delivered at home, 4,405 died, or 1 in 212.

These two sets of patients belong, according to Le Fort, to the same class of society; and, without entering into various questionable points connected with the table, we cite it here as sufficient to show—what is now generally acknowledged—namely, the greater danger to life which women undergo who are confined in lying-in hospitals, as these and other hospitals are at present constructed and conducted."[1]

If Le Fort has reached the truth as to the comparative merits of hospital and dispensary practice, he owes his success to chance, not to good management. To find the mortality of dispensary and hospital practice, he takes the statistics of a great variety of hospitals and dispensaries, adds them together, and gives the result. In my opinion, most of the statistics are worthless, and the results are of course equally so.

Le Fort says the mortality of childbed in home practice is 1 in 212. His statistics comprise the data, among others, of many dispensaries. Among these are such figures of mortality as the following :—1 in 595, 1 in 453, 1 in 390; and in one dispensary no mortality at all. Now, no obstetrician of common sense can believe that the mortality of childbed in any circumstances is 1 in 595, or 1 in 453, far less that a dispensary practice can be so successfully conducted as to have no deaths at all. The adding together of utterly incredible data cannot bring Le Fort to the truth (1 in 212). Incredible data mixed with credible can produce only incredible results. Le Fort's collection, as managed by him, is not only worthless; it is also misleading. I know of no dispensary data of home practice that can be relied on. I have had extensive experience in dispensary practice, and I could publish splendid-looking results: but I do not, because I cannot rely on them. They are, however, as good as any of Le Fort's. In the great debate in the Obstetrical Society of Dublin, opinions like mine, as to the value of dispensary data, were generally expressed.

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[1] *The Lancet*, Sept. 4, 1869, p. 332.

Let us now turn to Le Fort's mortality in hospitals, 1 in 29. Hospital data are generally accepted as reliable. Le Fort then has shown, that in a great collection of hospitals there has been a mortality in childbed of 1 in 29—a fearful, and I believe, a true statement; enough to condemn them in mass. Among the data of hospitals we find such mortalities as 1 in 7, 1 in 15, 1 in 18, 1 in 21, and so on—an awful tale. We do not need to compare these figures with the corresponding figures of dispensaries, even if we could rely on the latter. When mortality like any above given is known, there can be nothing of condemnation too strong; there can be no slander. The truth is too hideous for any attempt at defence. But because one hospital or several are bad, are all therefore bad? Because all hospitals have a mortality of 1 in 29, should an hospital whose mortality is 1 in 84 (Dublin), or 1 in 100 (Dublin), be condemned? Because one or several doctors are bad and injurious to the community, are all bad?—I trow not.

To judge hospitals aright, the proper course is, evidently, to take the best as an example. We cannot justly condemn all hospitals because in that of Leipzig the mortality is 1 in 7. We do not condemn ovariotomy because in the hands of some surgeons the mortality of it is nearly cent per cent. We take the practice of the best ovariotomists to judge by.

To judge of hospitals, let us take the Dublin Hospital as managed by Collins. In seven years he had 16,654 births under his care. The mortality of mothers was 1 in 100. When I take this example, I must add that I regard it as not the best, but as a fair, handy, and well-known example. It alone would have answered Le Fort's purpose better than all his collection.

If all hospitals are to be condemned, then this one must be condemned (1 in 100). And I ask any obstetrician to come forward and cast the first stone. Several have already come; but, in future, if any one is bold enough to come, he must tell us what is the mortality in his own practice, and he must consider whether or not his own practice is comparable with that of an hospital which receives the poorest, having among them drunkards, the diseased, the seduced, and also cases of difficulty in higher relative proportion than in ordinary practice.

The great Dublin Hospital, then, can show a mortality of 1 in 100. Sir James Simpson has published the mortality of two years of his practice; it is 1 in 45 at least. Dr. Crosse had in his practice a mortality of 1 in 98. I have had a mortality in mine of at least 1 in 105. Dr. M'Clintock's is 1 in 108. Dr. T. E. Beatty's is 1 in 121.

When we get really trustworthy and comparable figures, how does hospital practice look? I, at least, cannot condemn it. I see no reason to doubt that it may be, and has been, good enough to compare with any kind of practice. I know no good, large, and unexceptional data of deliveries, which show results that are better than those of Dr. Collins in the Dublin Hospital.

The Registrar-Generals' data,[1] when corrected for omissions and other errors, only confirm the conclusion derivable from the figures I have just cited. They show a mortality in childbed of not much less than 1 in 100. The Registrars' data are, however, and for many reasons, not so reliable as those I have adduced.

But I have not yet concluded my remarks on Le Fort and those who join him in attacking hospitals. They must be made to abide by their own figures and reasonings. The practice of hospitals, say they, shows results comparatively so bad that they must be condemned, and the practice of dispensaries shows results so good that they must be praised. Hospitals, say they, are very injurious, therefore shut them up; dispensary practice is very good, therefore extend it. They, selfdeluding, think they have shown dispensaries to have a mortality of 1 in 212—a smaller mortality than is anywhere else ascertained. Then, it is a rational and a necessary conclusion that they should recommend all lying-in women to espouse the poverty, the filth, the debasement, of the poorest classes, and to have a student, a midwife, or a neighbour to attend them. This, according to Le Fort and Sir James Simpson's method of reasoning, is the sure way of making a good recovery after delivery. In a well-managed hospital they will die at the rate of 1 in 29! In the slums of a great city they will die at the rate of 1 in 212! I do not rely entirely on figures; they are very deluding. But, as the enemies of hospitals have introduced their use, it is necessary to make a great correction of Le Fort and Sir James Simpson's figures and conclusions, and to say: In a well-managed hospital, they die at the rate of 1 in 100; all the country over, the mortality is probably not much less; in the best private practice it appears to be greater.

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[1] See page 19.

Figures cannot settle a question like that now before us. So far as they go, they appear to me to show a well-managed hospital to be worthy of admiration and encouragement. They should not diminish the zeal of all loyal physicians to introduce and carry out many needed improvements in them.

Statistics have often been used foolishly. They have, in my opinion, never been used more injuriously than they are now, and in this question.

CHAPTER X.

THE ASSERTED SUPERIORITY OF NEW HOSPITALS.

It is now alleged that newness is a quality so essential to a good infirmary that, in future, hospital buildings should be in the form of cottages, so cheaply constructed that they may be periodically destroyed, or built of such materials as iron plating, that they may be periodically taken down and reconstructed in a new locality, and thus kept always new. Rebuilding becomes necessary for any hospital when its materials become wasted from age and wear and tear, or its arrangements become antiquated. This is the case, at present, of the Royal Infirmary of Edinburgh. It is not to this kind of rebuilding that we are told to look for permanent newness and sanitary improvement. It is to a frequent demolition and rebuilding in a new locality, "every few years."[1]

This is a change in hospital arrangements so enormous, leading to so great expense, involving such great difficulties, that no wise man would propose it without reasons at once great, good, and cogent. We propose to examine the reasons assigned, and to inquire into the history of hospitals, to find out if that offers any evidence for or against the belief that there is greater salubrity in a new hospital than in an old one.

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[1] *Edinburgh Medical Journal*, March 1869, p. 817.

First of all, we must point out that the very proposal contains within itself evidence of weakness; for it is plain that such an inconvenient and expensive process as demolition and rebuilding, every few years, could never be proposed to a rational body of managers, unless the cottages every few years become insalubrious and dangerous to their inmates. The proposal, therefore, involves the very remarkable and weighty admission that cottages will become insalubrious and injurious every few years; so much so, that nothing short of destruction of them will mend matters. This is a cruelly self-destructive admission; and it comes from a defender of the cottage system. Yet, after all, there may be some virtue in newness; and we proceed to inquire if it is demonstrated. The demolition and rebuilding is to produce newness. Let us examine the arguments on which this portentous plan is founded.

"When," says Sir James Simpson, "the two largest hospitals in Scotland—viz., the infirmaries of Edinburgh and Glasgow—were opened in the last century, the buildings of which they then consisted were new and fresh, and comparatively small. In the Edinburgh Infirmary, out of the first 99 cases in which the limbs were amputated, 8 of the patients died, or 1 in 12. Out of the first 30 amputations of the limbs in the Glasgow Infirmary, 1 patient only died."[1] He then goes on to point out that these results far excel those that are now produced.

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[1] *Edinburgh Medical Journal*, March 1869, p. 817.

This statement contains, so far as I know, the only evidence adduced in favour of the quality of newness of an hospital. It is short enough for the gigantic conclusion built upon it; but though short it may be found to be pithy. We must examine it.

No authority or reference is given whereby the source of the statements might be discovered and their scrutiny facilitated. I therefore went to the chief official of the infirmary of Edinburgh, Mr. M'Dougall—the best authority, also, as to our infirmary documents. He at once told me that the early documents of the infirmary were, for statistical purposes, utterly valueless, on account of their incompleteness; that, so far as he knew, they had never been searched for amputation statistics; that a search, if made, would yield nothing of any value; and that the first statistics of the hospital on which reliance could be placed were with much difficulty made out by Dr. John Reid, who, it is well known, flourished long after the beginning of the infirmary.

This condition of the source of the Edinburgh evidence in favour of newness might render further consideration of it quite supererogatory. Nevertheless we proceed to notice another peculiarity of it—namely, the smallness of the figures. When a high number is not obtainable, one may be glad to have 99 (Edinburgh) and 30 (Glasgow). But in the present instance, the number that might be taken is limited only by the duration of the quality of newness—a period of time which is nowhere described. We therefore cannot explain the mystery of 99 and 30; nor can we dream of any explanation why 99 is the number selected for Edinburgh and 30 for Glasgow. Still another curious peculiarity lies in the numbers 99 and 30. Why were they chosen? Why not 50, or 100, or 150, or 200? These unexplained circumstances lead us to believe that the choice has been made with art, not with wisdom. But whatever may have been the method or principle of selection, any one can see that the figures are too small for any conclusion of value in the present question, especially when the smallness of the mortality is held in view; and that the doctrine of chances will account for far greater successes than are here recorded, without any resort to the quality of newness as an explanation.

We now examine the 30 of Glasgow Infirmary. These are derived from Lawrie's paper on the Results of Amputations.[1] I know little as to the value of the documents whence these data were derived. But their completeness may be guessed at if we remember that they go back to 1794, and if we notice what Lawrie said of them: "Many," says he, "of the journals have been lost, and others are very carelessly kept." The 30 amputations were "for disease," and they ranged over a period of seventeen years. Is an hospital seventeen years old still a new hospital? If it be not, what is the value of this 30?

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[1] *Medical Gazette*, 1841, p. 394.

The next point to be noted in regard to Lawrie's 30 amputations with 1 death is that Lawrie ascribed the success, not to newness of the hospital, but to the early performance of the operations. I give his own words. "The error," says he, "we at present commit is delaying amputation after every rational hope of cure has fled, merely because the patient is not obviously dying. The consequence has been the reduction of the success of amputations from 1 in 30 to 1 in less than 3 by one calculation, and from 1 in 9 to 1 in 3⅖ by another." In another place Lawrie adds: "It is, however, but justice to ourselves to state that the more dissipated habits and lowered condition of our patients, added to the increased size of our hospital and the crowded state of its wards, must have considerable influence in diminishing the success of our operations."

So far is Lawrie from thinking evil of the old Glasgow Infirmary, that he is careful to point out that, though the amputations were less successful, the number of cases of disease cured was as great in the old as in the new hospital. After stating that in the old times amputation was had recourse to in cases in which its mere proposal would in later times be scouted, he says: "Of the whole number amputated in the early period, 1 in 9 died; in the later, 1 in 3⅖. Cured, including amputations, in the early period, 40, or 1 in 1¾; in the later, 48, or 1 in 1¾ very nearly. Cured without amputation, in the early period, 13, or 1 in 5·4; later period, 31, or 1 in 2·6. From which it appears that the *number of cures* in the two periods is nearly precisely the same, but that in the early period it is effected principally by the amputating knife, in the later by treatment." Who can doubt that, in this respect, Lawrie thought the later excelled the earlier period? Who that looks with single eye for truth will fail to see that the 30 early amputations with 1 death prove nothing, till a variety of possible interfering conditions, such as Lawrie indicates, are shown not to have produced the seemingly favourable result.

To prove, by statistics, the value of the quality of newness, it is necessary to observe the greater success of new hospitals generally, or in a preponderating number of instances; and, further, to prove that like success is not attained apart from newness—is not obtained in old hospitals. Now, unfortunately for the argument in favour of newness, it is easy to show success in old hospitals equal to, if not greater than, 1 in 12, or 1 in 30; if the data of comparison do not rise above 99 or 30. For instance, Mr. Callender[1] states the occurrence of 78 consecutive cases of amputation of the upper extremities, without a single death in his large London hospital, above a hundred years old. Still more unfortunately for Sir J. Simpson's argument for newness, he has himself utterly destroyed and refuted it, probably in forgetfulness of having ever stated it. Nothing else can account for this example of "hoist with his own petard." Encountering Mr. Holmes's just incredulity as to the value of his statistics, he writes a passage in laboured self-defence,[1] which will repay the student's perusal as a good *reductio ad absurdum* of his own previous argument in favour of newness. I quote only a short part, adapted to my immediate purpose: "Now," says he, "at St. Bartholomew's Hospital in London, in 1855, 17 males suffered amputation, and all recovered. During the same year, in 25 amputations, 12 of which were thigh cases, there was only 1 death. In 1861 there were 24 consecutive amputations with only 1 fatal case. In this way, in that metropolitan hospital, during these periods, out of 49 limb amputations only 2 died, or about 1 in 25." Thus the hospital above one hundred years old had a smaller mortality than the Edinburgh Infirmary when new.

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[1] St. Bartholomew's Hospital Reports, vol. v.

But it would be a waste of time to pursue this subject further. The argument for newness is, so far as it has been given, destitute of force or ingenuity. When we consider the immense issues based upon it, we might justly characterise it in terms which we shall not use. To propose, on such grounds as we have above considered, that hospitals should be pulled down every few years, and rebuilt, is at least premature, perhaps also ludicrous.

As should be well known, Dr. Thomas Keith has recently had twenty-six consecutive cases of ovariotomy, with only one death. The operations were almost all done in some chambers that have been for many years used as a private hospital. What would be thought of the wisdom of any one suggesting that Dr. Keith's unparalleled success at the time referred to should be held as indicating that a comparatively old hospital is best, and that all other hospitals should be made like his? Yet such proposal would have better grounds than any Sir James Simpson gives for his, for Dr. Keith's success has never been equalled.

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[1] *The Lancet*, October 2, 1869, p. 476.

If the kind of reasoning which Sir James Simpson adopts regarding St. Bartholomew's in 1855, be admissible, then the facts, which he has himself adduced, show that an hospital should be one hundred years old before it is regarded as salubrious!

We now turn to a few hospitals, to inquire what they have to tell as to the influence of newness; and I take maternities, the kind of hospital with which I am most familiar. Such hospitals are, I believe, better adapted than any other for the study of questions such as that now before us. No doubt even in them there are difficulties arising from the varying proportions in different hospitals of primiparity, of difficulty in labour, of women under the depressing influence of seduction and of other adverse conditions; but at the same time labour is a distinct unity with well-known laws, and this cannot be said of amputations, with all their varieties of situation, of kind, and with their subdivisions into primary, and secondary, and others. I can give only a few examples of the influence of newness in maternities.

The Lariboisière is the newest large Parisian hospital. "All the world," says M. Depaul, "knows the excellent situation of this magnificent hospital; the wards are vast and well ventilated. The number of deliveries is inconsiderable; and, nevertheless, the cases of death by puerperal fever are proportionally very numerous." Everything is in its favour. Yet the deaths are, proportionally to other old Parisian hospitals, very numerous. The earliest statistics of it which I can procure, and probably of its very first years, show a mortality among the lying-in, from puerperal fever alone, of 1 in 24 in 1854, 1 in 22 in 1855, and 1 in 26 in 1856.

St. Louis is an old Parisian hospital. The mortality from puerperal fever in it was, from 1852 to 1856, 1 in 416. Puerperal fever alone is not an altogether satisfactory test of an hospital's salubrity; but, so far as it goes, it shows that the old hospital far surpasses the new one.[1]

The Edinburgh Royal Maternity Hospital was, in 1856, removed from Minto House to its present location. I have never heard that it has been otherwise than remarkable for insalubrity.

Lastly, we turn to the grand example of the Dublin Lying-in Hospital. It has existed for more than a hundred years. It is a large hospital, the largest in the British empire. Its total mortality has been 1 in 72. One of the most powerful attacks made upon the present hospital system is really a criticism of this hospital's mortality made by Dr. Evory Kennedy. He has published a table, with coloured figures, showing admirably the fortunate days of this great institution.[2]

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[1] *Fièvre Puerpérale: Communications*, etc., pp. 29 and 30.

[2] *On Hospitalism and Zymotic Diseases*, p. 30.

I leave readers to judge, from the following extracts, what influence newness and oldness have exerted in this hospital, which was first occupied in 1757. I give a few figures only, but sufficient to illustrate the present subject: "In 1757 the mortality was 1 in 55; in 1758, 1 in 50; in 1766, 1 in 227; in 1768, 1 in 41; in 1795, 1 in 214; in 1822, 1 in 220." The hospital, when quite new, had a mortality far above the average. When comparatively old, it had frequently long runs of great success.

In conclusion, I shall state what I have proved in this short chapter. I have not proved, nor do I believe, that newness is not a great advantage. But I have proved that newness is no guarantee of salubrity or of success, and that oldness is no barrier to even extraordinary salubrity and success. So universally is newness appreciated, in the ordinary sense of the word, that the citizens of Edinburgh have resolved to desert their old hospital, and build a new one in a new locality, at an enormous expense.

CHAPTER XI.

THE ALLEGED SALUBRITY OF SMALL HOSPITALS.

SMALL hospitals are said to afford to patients greater chances of recovery than large hospitals. Cottages are said to afford still better chances than small hospitals. Therefore, say the enemies of hospitals, large and small hospitals should be supplanted by a system of cottages. The reasoning is specious, but the proof rests mainly on statistics. While I cannot admit the validity of the kind of statistical reasoning adduced against hospitals, I purpose now to inquire how far mere statistical data confirm the view that small hospitals are better than large ones.

It will be readily and on all sides admitted that the data of maternity hospitals offer us an excellent means of conducting this inquiry, and I propose to confine my observations to obstetrical data. There are various ways of pursuing the subject. To be comprehensive, I shall employ three methods. *First*, I shall examine the data of one small hospital; *second*, I shall examine the data of a promiscuous collection of small hospitals; *third*, I shall examine the data of some highly praised small hospitals.

THE DATA OF ONE SMALL HOSPITAL.

In a question like that now before us, a careful and shrewd inquirer is sure, if he have a convenient example of a small hospital, to examine its results first of all. Nothing is more convincing to the man of common sense than what he personally knows and inspects. He cannot know many hospitals intimately, but he may know one. Now, for me, there is a convenient hospital for the present purpose—namely, the Royal Maternity Hospital of Edinburgh. It happens to be particularly valuable in the present crisis, remarkably appropriate for my purpose, because it has been under the care of a body of distinguished obstetricians, with Sir James Simpson, a considerable party in the present question, at their head. This gentleman accepted a special yearly vote of thanks for his assistance in the management of the institution and the treatment of the patients.

The hospital is a small one, for it has only about twenty beds occupied at once, and its annual number of deliveries is considerably below 300. The celebrated obstetrical statistician, Dr. Churchill, says,[1] that during the years 1844 to 1868, there were in it, 3824 deliveries and 62 deaths, or 1 in 61.[2] During the year 1867, there were 259 women delivered in it, and 6 deaths, or 1 in 43. This is a very high mortality. The great Dublin Hospital, the largest in Great Britain, has, in an experience of above 100 years and above 190,000 cases, had a mortality of 1 in 72. During the famous mastership of Collins, lasting seven years, and an experience of above 16,000 cases, the mortality was 1 in 100.

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[1] *Dublin Quarterly Journal of Medicine*, August 1869, p. 251.

[2] On this figure see some remarks by Dr. Charles Bell. *Med. Times*, Nov. 20, 1869, and rejoinder by me in the number for Nov. 27.

This looks dark for small Hospitals. One, that may be called Sir James Simpson's, presents a picture which would have deterred most men from asserting that small hospitals are more salubrious than large ones. But all this does not settle the question. It only shows that smallness is a quality of an hospital which offers no security for superior so-called salubrity. It shows this irrefragably. Other individual small hospitals might be cited to show the same thing.

A PROMISCUOUS COLLECTION OF THE DATA OF SMALL HOSPITALS.

I am happy to be able to refer to two such masses of statistical data, gathered respectively by Dr. Churchill, the obstetrical statistician, and by Le Fort, the enemy of hospitals.

Dr. Churchill's collection is from eleven sources. It comprises 27,300 cases. Among these were 405 deaths, or 1 in 67.

How do small hospitals look? 1 in 67! The great Dublin Hospital mortality is 1 in 72. Drs. Collins, and Johnston, and Sinclair, in 28,528 cases, which they had in this large hospital, had a mortality of only 1 in 123. But small hospitals, according to Churchill, have a mortality of 1 in 67. According to the same authority, large hospitals have a mortality of 1 in 62. It is necessary, however, to remark, that this last mortality of large hospitals is only arrived at by Dr. Churchill including in his data figures derived from such institutions as are to be found in Paris and St. Petersburg, having mortalities quite extraordinary, and far above anything known in this country.

Le Fort's collection is from 31 sources. It may be epitomised as follows:—

TABLE XXVI.

Hospitals.	Cases.	Deaths.	1 in
Institutions receiving annually above 2000 lying-in women	300,503	12,244	23
Institutions receiving annually from 1000 to 2000 . .	39,885	1,444	27
Institutions receiving annually from 500 to 1000 . .	14,393	386	37
Institutions receiving annually from 200 to 500 . .	9,755	299	32
Institutions receiving annually from 100 to 200 . .	4,558	126	36
Institutions receiving annually under 100 . . .	548	46	12

What, then, does this Le Fort method of calculating tell regarding small hospitals? It makes them to be the worst of all. The smallest have the most frightful mortality. Naturally, Le Fort says, when describing the data which we have above given, "the statistical researches are there, to prove that one must not seek the cause of the greater or less mortality of the lying-in in the size of the establishments, and that all hospital hygiene cannot be reduced to questions of architecture. . . . It is easy to see, in running over the figures, that the relative mortality in the small and large maternity hospitals does not formally plead in favour of the small establishments."[1]

Whatever may be said of much of the reasoning that Le Fort has used, there can be no doubt of the wisdom of the passages we have just quoted. We leave them without further comment.

THE DATA OF SELECTED SMALL HOSPITALS.

I take the selection made by Dr. Evory Kennedy, because it is made by a friend of small hospitals. I myself know of no data of small hospitals that are reliable, else I would have taken them in preference to the selection of Dr. Evory Kennedy. The cause of my want of reliance is simply the smallness of the number of cases collected. This circumstance damages Le Fort's general collection, as well as Dr. Evory Kennedy's selection.

Dr. Kennedy's selected examples are the following:—[2]

TABLE XXVII.

Cottage Hospitals.	Annually.	Total.	1 in
New Ross . .	30	924	185
Waterford (23 years)	115	...	295
Limerick . . .	367	...	367

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[1] *Des Maternités*, p. 75.

[2] *Dublin Quarterly Journal of Medical Science*, May 1869, p. 295.

Now this selection by Dr. Kennedy certainly has an aspect favourable to the view that small hospitals are very salubrious. But it must be scrutinised, and then objections to the validity of its evidence are easily found.

It proves too much for the enemies of hospitals, for the mortalities recorded are smaller than those in the practices of the best private practitioners. Private practice can, I believe, nowhere show such fine results as these little Irish hospitals. The Dublin practitioners, as a whole, have a mortality above 1 in 114. The Edinburgh practitioners, as a whole, have a mortality probably nearly the same as their Dublin brethren. There must be something misleading in Dr. Kennedy's selection. If it prove anything, it is that hospitals are safer for lying-in women than the cottages or dens of the poor, the houses of the better classes, or the mansions of the rich. In short, it is a selection that does not command the confidence of the intelligent obstetrician. There is plenty of evidence to prove that a mortality of 1 in 295 or 1 in 367 has never, on a large scale, been known in this world.

The selection further presents us with too small numbers. The very small mortality shown in Dr. Kennedy's selected hospitals will certainly be corrected as time rolling on increases the numbers to judge by. The whole number of cases in the three hospitals is probably about 4000. It is easy to extract, from the great Dublin Hospital's experience, data of consecutive years as splendid as those of the three small selected hospitals, and which might be quite as justly applied, to indicate its mortality, as those of the three small hospitals are held to indicate the salubrity or mortality of the little institutions; and the citation will serve as a good example of the danger of small selected figures. In 1807 and 1808 there were delivered, in the great Dublin Lying-in Hospital, 5176 women; of these 25 died, or 1 in 207.

In conclusion, then, we find that the statistics of maternity hospitals afford no countenance to the notion that small hospitals are better or more salubrious than large. Indeed, they at least appear to show that the reverse is true. They, however, indisputably prove that smallness is no guarantee of success or salubrity.

It is a matter of course that a small hospital is likely to be more salubrious than a large one. A small family is more easily managed than a large one. But there is no evidence that the difficulties attending the management of a large hospital or a large family are insuperable. And just as a man may not find it prudent to break up his large family into two or more establishments, so a community may not find it prudent to break up the mass of their sick poor, and lodge them in two or more small establishments, instead of one large hospital.

In an address published in a daily Edinburgh newspaper, Sir James Simpson speaks as follows:[1]—"When," says he, "such a simple operation as amputation of the forearm is performed upon a poor man in the country and in his own cottage home, only about 1 in 180 dies; but the statistics of our large and metropolitan hospitals disclose the stern and terrible truth, that if these men had been inmates of their great wards, thirty of them, or about 1 in 6, would have perished."

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[1] *Scotsman*, October 27, 1869, p. 7.

All our cherished Edinburgh hospitals depend mainly for their support on voluntary contributions. The sick poor are tended, medical education is carried on. Can any further support be expected from that portion of the public who may unfortunately believe that the above quotation is even an approximation to the truth? I have, in this short paper, tried to find out what is the truth as to large and small hospitals in a department of medicine with which I am somewhat familiar. My readers will then easily understand how sadly I deplore the appearance of such statements as I have quoted. I know nothing that, if only listened to, is fitted to be more injurious to the sick poor and to the medical profession.

CHAPTER XII.

HOSPITAL *VERSUS* HOME PRACTICE.

THIS important question is now so fully occupying the attention of the profession, that its nature and general bearings do not need to be pointed out. The decision of it is made to depend upon statistical statements and reasonings to a degree which I cannot approve. I think that other considerations bearing upon the question deserve much more attention and respect than they get; but at present I shall not even enter upon them. I propose to consider the statistical statements that are adduced, and to whose sole arbitration the profession is asked to bow.

Now, the first thing to be considered in a statistical statement is the worth or reliability of the data. I do not mean to inquire into, far less dispute, the desire on all hands for truth. I mean to inquire into the value of the data brought forward as the foundation of true conclusions. If the data be true, a great point is made out. If the data be false or not trustworthy, the whole statistical reasoning is false or not trustworthy. If the data be true, it does not follow that the conclusions drawn from them are valid or true. Before that is admitted, the reasoning also must be carefully scrutinised.

Two great sets of data are brought forward for comparison—hospital statistics, and statistics of home practice. They are further subdivided into surgical and obstetrical data.

Hospital statistics, whether surgical or obstetrical, are generally accepted as reliable. I know of no serious challenge of their truth. Nothing further need be said of them. Only, it may be added that their very alarming aspect, at least at first sight, is favourable to the idea that they are true; for institutions, like individuals, have a tendency to make things look pleasant—a quality in which hospital statistics are certainly deficient. Quite recently,[1] an attempt has been made to injure the character of hospital statistics of amputations. While I have no doubt that hospital statistics are imperfect, I have also no doubt that all other statistics are very much more imperfect and unreliable. If hospital records be not trustworthy, what faith can be placed in a collection of private records such as Sir James Simpson offers as the basis of his arguments and conclusions?

The statistics of home practice are in a very different position from those of hospitals; and it is a very serious and indispensable duty at present to inquire into their value. The statistics of home practice are got from individual practitioners, or private practice, and from dispensary or home practice of public institutions. In order that any confidence in such data may be expected, the following conditions are necessary:—

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[1] *Lancet*, October 2, 1869, p. 477.

1. They must not be derived from memory.[1] It is almost useless to insist on this point. No one, even very slightly acquainted with human nature, will trust the memory in such circumstances. Trusting this frail record, I have repeatedly heard practitioners of large experience in midwifery say, that they never had a death in childbed; and this even when they condescended on the number of their cases. Trusting this frail record, I have heard practitioners of large experience say that they never saw any great evil or death result from the use of intra-uterine pessaries; yet, very fortunately for the female sex, this boasted remedy has fallen into almost entire disuse. Trusting to this frail record, let us hope, is part of the explanation of the present delusion of the profession as to the safety and efficacy of the treatment of dysmenorrhœa and sterility by splitting the cervix uteri. Trusting to this frail record is probably the explanation of the great success at one time attributed to iodine injections in ovarian dropsy. Examples of the evil abound. Had men noted down and numbered their cases, we should never have had the succession of therapeutical delusions, calamitous to the female sex, which have risen and fallen of late years. Any man who does not note down and number his cases should, in the present day, when the value of statistics is admitted, be ashamed of

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[1] "All conclusions drawn from the memory are," observes Malgaigne, "horribly fallacious; and it is," he adds, "to their employment that we owe the astonishing delusions almost generally professed regarding the real danger or fatality of amputations."—Simpson's *Obstetric Works*, vol. ii. p. 546.

coming forward as recommending new remedies. Certainly he cannot be implicitly trusted. Sir James Simpson believes all statistics of amputations derived from memory to be "horribly fallacious."[1]

2. The second condition necessary is, that the items should be noted down at the time of their occurrence. In the present investigation, two items require to be noted down: first, the operation or the confinement; and second, the recovery or death. Items noted down after a lapse of time are nearly as misleading as if the memory, entirely unaided, were trusted to. This noting down of two items at separate times leads to a demand for great care; and I am satisfied that, in the case of public institutions for home practice, this care is not always taken; and the result is data of no value. Take obstetric practice of dispensaries. Cases are all entered when allotted to different midwives or students; but the entry of the delivery is often omitted, and the upshot of the whole case is often not recorded. The midwives or pupils forget this duty, or go away from the place, and cannot fulfil it. This is the way in which my own dispensary statistics are rendered unsound, and it is the best account I can give of the manifest imperfections of dispensary statistics elsewhere. In like manner, in surgical practice, I can easily conceive that an operator, especially if he

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[1] Regarding the statistics here specially referred to, see a letter at p. 94 of the *Edinburgh Medical Journal* for July 1870. Were I disposed to enter into details, I could adduce other examples of numerical error in the same tables. I here confine myself to general statements.

operate far from his abode or for a neighbouring practitioner, never hears the upshot of the case, or sometimes forgets it if he does.

3. The third condition is, that the statistics of dispensaries or of private practice yield credible results. This may appear to some a supererogatory demand; but I am sure it is not, and I shall show it. Again, I shall take as an example the statistics of midwifery dispensaries. Le Fort and Sir James Simpson proclaim a mortality of 1 in 212 in the home practice of midwifery. I shall show that this result is absurd, by an appeal to the absurdity of the data on which it is founded. Preliminarily, I may say that, so far as I can make out, ordinary midwifery practice in good hands yields a mortality of not less than 1 in 120. In its particularly fortunate years, the Dublin Hospital has seldom gone far above this. Private practice on a large scale goes above this, probably, quite as seldom. But Le Fort and Simpson ask us to believe that the average mortality of home practice of midwifery is 1 in 212. This is a very small part of what they ask us to gape and swallow, shutting our eyes. The data upon which this mortality of 1 in 212 is based contain, among others, the following stupendous successes.[1] The Westmister Benevolent Institution, in 4761 confinements, lost 8 mothers, or 1 in 595! Who is to believe this? How can practitioners, who generally lose about five times as many, believe it? How can a

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[1] *Des Maternités*, p. 32.

gentleman,[1] who in two years of his private practice loses 1 in 45, believe it? Here is another of Le Fort's items. The Stettin Polyclinique, in 375 cases, lost 0. That is still more wonderful, and likely to be fruitful in equally wonderful conclusions, when it is the basis of an argument. The Westminster General Dispensary, in 7717 cases, lost 17 mothers, 1 in 453! Come from such statistics of Le Fort and Simpson to the sad statistics of comparatively rural practice. Mr. Ellis[2] and his brother had 2157 cases of labour; they lost 19 mothers, or 1 in 113. I am sure the Messrs. Ellis cannot find the mortalities above recorded to be credible by them. Mr. Harrinson, in 1000 cases, lost 1 in 111.[3] How can these gentlemen be expected to give credit to the extraordinarily small mortality above recorded? If they do give credit to these remarkable statistics, they must also be very sorry that they acquired skill and experience and got practice among well-to-do people. They must long, for the sake of their patients, to have the filth, the bad air, the poverty, of London dispensary practice, imported into their rural localities; and with them the imperfectly educated midwives and medical students who achieve the Baron Munchausen successes.

I am not a surgeon, and must speak with some diffidence on an unsettled surgical point; but I cannot avoid saying that some of Sir James Simpson's surgical data are very like the incredible obstetrical data which I have just given. For example, he is in my eyes a marvellous, or, to use a term of Mr. Holmes's, a "miraculous" surgeon, who has performed fifty-two primary amputations without a death; yet he finds a place in Sir James Simpson's statistics.[1] His name is not given.

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[1] Simpson's *Obstetric Works*, vol. ii. p. 639.

[2] *British Medical Journal*, January 22, 1859, p. 64.

[3] *Ibid.*, November 12, 1859, p. 909.

4. The last desirable condition of good statistics is, that they should be collected with no particular object in view. The tendency is to produce the kind of figures that is wanted; and for this there are several pretty evident reasons. No one, so far as I know, has so often as Sir James Simpson dwelt upon the advantage of a statistical collection being made without a view to an argument, or with a view to oppose a favoured opinion. These advantages are certainly carefully excluded in the present argument, so far as private practice goes; for, in his letter[2] to his country friends, asking for their data, Sir James Simpson has reiterated the opinion which he hopes to prove by the data for which he asks.

To conclude: I find the new statistics adduced in the present argument against hospitals to be worthy of no confidence. For, first, we have no assurance whatever that the statistics of amputations in private practice are not derived from memory, and therefore "horribly fallacious;" second, we have no assurance that the items were noted down by the observers at the time of their occurrence, in the statistics which are peculiarly relied on by Le Fort and Simpson; third, some of the data in the peculiar statistics of the same gentlemen are "miraculous" or incredible; fourth, the collection of amputations from private practice is damaged by the very terms of the letter which led to their return.

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[1] *Edinburgh Medical Journal*, March 1869, p. 827.

Ibid., March 1869, p. 820.

CHAPTER XIII.

HOSPITAL *VERSUS* HOME PRACTICE.

(*Continued.*)

As in the last chapter, devoted to a consideration of the value of the data adduced by the enemies of hospitals, I have not expressed my own opinion about the merits and demerits of these institutions; so, in this chapter, I shall consider the reasoning (against hospitals) into which the above-mentioned data are introduced, without entering on their good and bad qualities.

I must, preliminarily, express my disapproval of attaching to them the designations of "palaces" and "palatial structures." This is either a monstrous error of description, or it is a miserable specimen of grim and thoughtless irony.

A statistical argument is entirely, or almost entirely, based on figures. In the natural course, then, its conclusion demands and obtains unanimous assent, if there be no flaw in the reasoning. It is nearly like a mathematical demonstration. When, therefore, we find the conclusions of a statistical argument, such as that against hospitals, meeting with incredulity and opposition, we have to go back to the arguments in order to consider the data and the reasoning. The former I have already discussed; I now turn to the latter.

Before doing so, I may remark that students often find the conclusion of an argument so outrageous, that they do not stop to scrutinise the data and the reasoning. They simply laugh at the conclusion and reject it. This process may very justly be adopted with Le Fort and Simpson's conclusion that, in the home practice of maternities, only 1 in 212 dies. An obstetrician, knowing that such favourable results are nowhere obtained, naturally says he cannot believe that they are now discovered among the poorest of large towns. When he turns to the details of the statistics, and finds such mortalities of childbed as 1 in 591, his rejection of the conclusion becomes derision of it. A like process of rejection of some of Sir James Simpson's surgical statistics is adopted by Mr. Holmes, who regards some of the successes recorded as being, in the eyes of a London surgeon, scarcely explainable, except by supposing a miracle. I shall give another example from Sir James Simpson, of an obstetrical conclusion adopted by himself,[1] in which he says the statistics "offer a kind of evidence which is not less remarkable for its intelligibility and simplicity than for its precision and certainty." I am sure that, had he only thought of it, he would have taken the vaunted statistics as a mere sample of incredible nonsense, masked by a solemn numerical statement. The statistics referred to are said to prove that, from 1660 to 1820, the mortality of childbed diminished from 1 in 44 to 1 in 107. It is needless to say that the statistics adduced are so imperfect in their nature as to prove nothing whatever; and that the statistical result in question gives such a magnificent view of the progress of the practice of midwifery, that it is at once rejected as beyond belief, without inquiry into the errors of the supposed proof. Would to God it were true; we might then hope soon to see childbed despoiled of all its dangers. We cannot even now tell what is the mortality of childbed in London. How Merriman and Simpson find out what it was in 1680 and onwards, when there was no registration of births, is an inquiry that might be amusing, certainly not instructive.

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[1] *Obstetric Works*, vol. ii. p. 544.

I shall give another example of a conclusion similarly deduced, and to be rejected without scrutiny of data and reasoning. Sir James Simpson statistically demonstrates that first tappings of ovarian cysts are very dangerous—that 1 in 5 dies[1] from the operation. Who that has any matured experience can believe this? Now, when it is known that Wells and Keith can perform ovariotomy with the same mortality, or less, who will think it worth while to inquire into the nature of the statistical data and reasoning which are vainly imagined to establish the paradox?[2]

The argument against hospitals is conducted as follows:—The mortality of amputations and confinements is greater in hospitals than in home practice. The patients differ from one another in no important respect except the point of hospital or home residence; therefore, hospitals are murderous, and should be revolutionised or closed. If the argument here is all right, then the conclusion is fair. If the two first steps are valid, then the result is true. We do not doubt that the first step is valid, that the mortality in hospitals is greater than in home practice. This has long ago been often pointed out. But we refer to the previous chapter for our reasons for rejecting Le Fort and Simpson's evidence on this point.

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[1] *Obstetric Works*, vol. i. p. 266.

[2] See *Lancet*, Feb. 28, 1857.

The second step is, "The patients differ from one another in no important respect except the point of *hospital* or *home* residence." If this is not true, if this is not admitted or proved, then the conclusion against hospitals is not fair. The argument fails. Now this second step is neither proved nor admitted. So far is this from being the case, that the triumphant declaration of the conclusion against hospitals must be regarded as a burlesque upon reasoning.

Many differences between hospital and home patients have been pointed out. It is notorious that the mortality of towns is much greater than that of country districts, in which are the homes of most of the patients now under discussion. It is notorious that many of the worst or most hopeless cases are sent from the country to the town hospitals. It is notorious that hospital surgeons are bolder in trying to rescue by operation than home practitioners. It is notorious that the most poor, wretched, unhealthy patients find their way to hospitals in extraordinary numbers. These are a *few* differences between home and hospital practice. They are enough to invalidate the argument against hospitals. Even if they be not admitted, the difficult task of proving that they are not true remains for the enemies of hospitals as a preliminary to the acceptance of their argument against these institutions. It is incumbent on them to *prove* that residence is the only important difference between home and hospital patients. This they have not done, and, so far as I see, cannot do.

It is easy, and it may be instructive, to illustrate, by example, the viciousness of the reasoning which is employed in the argument of Le Fort and Simpson. The mortality of childbed in Edinburgh, as got from the Registrar-General's returns, is about 1 in 160. In the rural districts of Scotland it is about 1 in 200. The patients are in both regions in the same conditions, except that in Edinburgh there is a great aggregation of experienced and learned obstetricians; in the rural districts, practitioners are much segregated. The obstetricians of Edinburgh are, therefore, more dangerous to their patients than those of the country. They should be separated from one another, and deprived of their learning and experience. Now the error in this argument lies in the arbitrary assumption that the two sets of cases considered differ only in regard to the medical attendance. In like manner, the error in the argument against hospitals lies in the arbitrary assumption that hospital cases differ from home cases only as regards residence in the hospital or at home.

There is another error in reasoning on the subject under discussion into which Le Fort and Simpson have fallen. They take the data of all hospitals—good, bad, and indifferent—and use the results of the combined data to damage the character of every hospital. For example, they say that maternity hospitals have a mortality of 1 in 29. To this I reply, that no good maternity hospital has so high a mortality, and that such should be spared the disgrace of being placed in connection with hospitals which have. To judge fairly of an hospital, it is only necessary that it should show a large enough and long enough trial to justify a judgment. The Dublin Hospital has been in existence more than one hundred years. There have been more than 190,000 deliveries in it, and the mortality has been 1 in 72.[1] Is it proper, generous, or fair, to couple the good name of this institution with a disgraceful mortality of 1 in 29? to malign maternity hospitals as a whole because some are abominable? No doubt 1 in 29 is the mortality of maternity hospitals as a whole. But what has that to do with the question whether a maternity hospital, if well conducted, is a valuable institution or not; whether it yields better or worse results than home practice? As a help in settling

[1] *Dublin Quarterly Journal of Medical Science*, vol. xlvii. 1869, p. 293.

these important questions, it is grossly misleading. The statement of a mortality of 1 in 29 as the mortality of maternity hospitals is a slander on such hospitals as that of Dublin. The Dublin Hospital can show, for a period of seven years, with above 16,000 cases, a mortality of less than 1 in 100—a mortality probably nearly as small as that of the best contemporaneous practitioners in Dublin, and perhaps still more remarkably small if the character of the cases in the hospital be kept in mind. These last appear to me to be indisputably the kind of data fitted to be the basis of a judgment regarding the value of maternity hospitals.

What would be thought of a surgeon, wishing to estimate the value of ovariotomy, who took as his *data* the mortality of all the cases of ovariotomy he could find, done by any surgeon, or by any method? He would not be listened to. In his data would be found the experience of many men who never succeeded at all, every one of whose cases proved fatal. Such a collection would tell the actual mortality of ovariotomy, how many it had slain, how many it had saved. But it would tell nothing, nor help to tell, as to the value of ovariotomy. Such statistics might, if Le Fort and Simpson's reasoning were imitated, lead to an argument against ovariotomy as too dangerous to be ever justifiable. The proper data for judging of the value of ovariotomy are those from the large and long experiences of Wells and Keith. Judged by the Le Fort and Simpson argument, it might be condemned as murderous; judged justly, it is commended as a triumph of modern surgery.

It has long been well known that the mortality in hospitals, from all sources, is greater than in home practice. Farr in this country, and several distinguished foreign authors, have strongly insisted on this. It has been generally regarded as in a considerable degree depending on hospital arrangements being imperfect. While there can be, in my opinion, no doubt as to the injurious influence of bad hospital arrangements, or even of those in common use, there is equally no doubt that they are only one of several causes of high mortality in hospitals. To how great an extent this injurious influence operates no one knows; and certainly the evidence of Le Fort and Simpson is so little trustworthy and so ill argued as to contribute nothing towards the solution of the difficulty.

Some hospitals are extremely bad. It is related, for example, that Dupuytren lost every patient on whom he performed amputation in the Hôtel Dieu during an occupation of Paris. The Clinical Maternity Hospital of St. Petersburg is said to have had a mortality of 1 in 11. Of such institutions there can be no hesitancy in condemnation. They should be revolutionised or abolished.

Some hospitals are extremely bad at certain times. This is generally the result of overcrowding. Pyæmia, erysipelas, hospital gangrene, flourish. Such hospitals should be shut for a time, and the overcrowding or other discovered cause of the evil days be absolutely prevented in future.

Some hospitals cannot be shown to be in a marked way prejudicial to the health or recovery of their inmates. For example, the great hospital for lying-in women in Dublin has frequently and for series of years had a mortality probably nearly as small as that of the practice of the best practitioners in Dublin. Such hospitals should be imitated and admired. The causes of their diminished healthiness at certain times should be investigated, and, if discovered, removed in future. It must be remembered that private practitioners, as well as hospitals, have bad times in their practices.

All hospitals which I have seen are very imperfect. They should be improved. Medical men should combine to discover the causes of their imperfection, and the requisite remedies. To adopt notions as to the necessary insalubrity of hospitals, and the salubrity and advantages of cottages, would, in my opinion, be the adoption of mere whims, leading to a reckless and injurious mode of proceeding. It would lead to the destruction of invaluable institutions, without securing compensating advantages of any kind. If cottages are shown to have, on the whole, advantages over hospitals, then I am sure the profession will adopt them, and energetically set about to overcome the great and easily-seen disadvantages which the cottage system would necessarily entail.

I lately visited the magnificent hospital of Leeds. The wards seemed to me overcrowded. Whether this be so or not, the managers can, if they find the hospital unhealthy, improve it to almost any extent, by thinning out beds from the various wards. Till all this is done, and a great deal more, they need not think of pulling down the house and building cottages.

CHAPTER XIV.

HOSPITAL *VERSUS* HOME PRACTICE.

(*Continued.*)

THE comparison of hospital and home practice would, of course, be quite easy if satisfactory data were obtainable. The difficulty lies mainly in getting good data of home practice. This difficulty is greater, I believe, if amputations are made the special subject of comparison, than if deliveries are used for the purpose. I have already given reasons which, added to those of Messrs. Holmes[1] and Callender,[2] are sufficient to demonstrate the unsatisfactory and incomplete character of the amputation data which have been recently used to show the superiority of home practice. Now, I propose to try what can be done, with the view of contrasting hospital and home practice, if we use confinements for the purpose.

There is no difficulty in getting good hospital data. I shall use those of the Dublin Hospital alone. It is the largest and the oldest lying-in hospital in the British empire, and it has a vast mass of reliable experience to present in a statistical form—above 190,000 cases in the course of above 100 years. Here it is well worth while to remark that the data of an hospital with well-kept records are far more valuable than those of home practice. If the hospital be large enough, its unmixed data, being sufficient alone, have a unity that cannot exist in a mass of little scraps of private practice sewed together to make a whole for fit comparison. Further, the whole of the hospital's results are to be got. A private practitioner may not be able to give the whole even of his small experience. The bad times of the hospital are recorded as well as the good. The private practitioner, who has had bad times in practice, has an indisputable tendency and an inalienable right to maintain reticence regarding them. The practitioner with fine results is not unlikely to give them to the world, whether he be asked to do so or not. The practitioner with bad results is not unlikely to keep them to himself, even when asked by a friend to give them up.

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[1] *Lancet*, August 7, 1869. [2] *St. Bartholomew's Hospital Reports*, vol. v

The results of the home practice of midwifery may be arrived at in several ways. I shall describe them. They are all indirect and inexact when compared with the scrutiny of hospital books.

First :—A lower limit can be got; that is, a figure which certainly indicates a less mortality than the real one. The value of this is easily shown; for, if an author make the mortality of his own practice, or of lying-in generally, under the lower limit, then it is certain that his own practice has been erroneously given, or is quite exceptional, or that his estimate of the mortality of lying-in generally is erroneous.

This lower limit is obtained by reference to the reports of the Registrar-General. Under the heads metria and childbirth can be got a number of deaths of childbed, which, it is known, do not comprehend nearly all the deaths *in* childbed, which last is the figure that is wanted. The deliveries can be nearly exactly estimated from the number of children born, and the proportion struck. No doubt there are various sources of inexactness and inaccuracy in all this, but still the value of the figure obtained in this way is generally admitted, and it is certainly nearly as valuable as any used in the whole of this inquiry.

Many grave errors would have been avoided in the discussion of the question now before us, if the value of this lower limit had been known. I shall illustrate this by reference to the work of Dr. Evory Kennedy on Hospitalism, etc. Dr. Kennedy is a Dublin practitioner. The Registrar-General's report shows that the Dublin practitioners generally have had a mortality in their confinement cases of at least 1 in 114. That is the lower limit. Metria and childbirth deaths alone bring the mortality up to 1 in 114. As there are many deaths in childbed not included in either of these categories, it is quite certain that the mortality in childbed in Dublin is higher than this lower limit—1 in 114. Now, Dr. Kennedy, in the work referred to (p. 78), states his belief that 1 in 256 may be held to be the mortality in childbed, a figure to be contrasted with the mortality in the Dublin Hospital—a manifest and great error. The hospital is naturally expected to have a higher mortality in lying-in than that of the city generally. The city generally has a mortality above 1 in 114. The figure therefore suited for comparison with the hospital mortality is considerably above 1 in 114. 1 in 256 is thus shown to be an utterly misleading figure for any comparison or use.

Now, the lower limit for Edinburgh, as obtained from the Registrar-General's reports, is 1 in 160. For the towns of Scotland it is 1 in 178 (from returns of 1860-64).

Whatever may be the mortality of lying-in in private practice in Edinburgh, it must be above 1 in 160. In the towns of Scotland it must be above 1 in 178.

Second :—The Registrar's reports may be corrected so as to bring up their statements of deaths of childbed (metria and childbirth) to the statements wanted—namely, deaths *in* childbed (from all causes). If this can be satisfactorily done, then we shall have a statement of the mortality in childbed in home practice.

Now, this has already been done in two ways. Dr. M'Clintock[1] has ingeniously suggested that it should be done on the following plan. He finds the figure indicating the excess of the mortality *in* childbed above the mortality *of* childbed (metria and childbirth deaths), and adds this to the mortality *of* childbed. In other words, he finds how many lying-in women die of other causes than metria and childbirth, and adds the figure to that indicating the deaths from these causes. I have no objection to this method of calculating when no better is available. But I have used a more direct and safer plan, that is, to search the public records for all deaths of lying-in women from whatever cause. Some years ago I had this carefully done for Edinburgh and Glasgow in 1855. I did not limit the search for deaths to four weeks after delivery, but extended it to six weeks; and this is unfortunate, as four weeks is the usual time understood to be adhered to in inquiries like the present. But the error thus introduced must be very trifling and unimportant.

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[1] *Dublin Quarterly Journal of Medicine*, August 1869.

Now, this search showed that the home practitioners of Edinburgh and Glasgow in 1855 had, among lying-in women, a mortality of 1 in 107.

Third :—The third source of knowledge of the mortality in private practice is spontaneously published records. These form, for reasons already given, a source of data that is not very valuable. "There," says M. Danyau,[1] "there are secrets which every one keeps carefully to himself. If all the world were sincere," he adds, "perhaps we would find that the results are less favourable than is generally supposed." Accordingly, in the discussion in which Danyau took part, neither he nor any one else said what was the mortality in his private practice, although every one wished to know. Some darkly hinted at the greatness of the mortality, and there can be no doubt that the restraint arose from fear of revealing a high figure of deaths. The great Dublin discussion revealed some speakers bold enough to state the figure wanted. But the originator of the discussion satisfied himself with observing that he lost only 1 in 1200 from puerperal fever in his private practice: a statement equally valueless and misleading, hiding facts, not revealing them. To conclude, there can be no doubt that records of private practice will have a strong tendency to error in the way of representing the mortality as smaller than it really is. The data of unfortunate practice will come to light in unduly small proportion.

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[1] *Fièvre Puerpérale*, 1858, p. 177.

In private practice that is not exceptional, and in the records of which we have reason to think all deaths are included, I find such mortalities as the following:[1]—1 in 45, 1 in 56, 1 in 105, 1 in 108, 1 in 121, 1 in 159, and so on.

If we had the mortality of the practice of the practitioner who in 1852 flourished in Brackel, or the figures of those gentlemen who have had what is called an epidemic of puerperal fever, we should have high mortalities compared with those just recorded.

Now, let us come to a conclusion in regard to the mortality of lying-in women in private practice. I cannot, after all the care taken, be otherwise than indefinite. I can see no reason to suppose that the figure in private practice is far from 1 in 100. It may be worse. It may be better.

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[1] *Edinburgh Medical Journal*, November 1869, etc.

This has to be contrasted with the total mortality in the Dublin Hospital, 1 in 72; or with the mortality of large periods, as that of Collins, 1 in 100.

Considering the extraordinary aggregation of cases of difficulty and danger in such an hospital as that of Dublin, I can find room for no astonishment at its mortality. I can find no sufficient grounds on which to lay any blame at its door. I can find no reason to believe that the mortality among its lying-in would have been less had they been confined in their own homes; while I am certain that the loss in comfort and other valuable objects would have been immense.

The eminent Dr. Stokes has summed up the whole case of hospital *versus* home practice in a few words, and in favour of hospitals. The mortality in private practice is, he believes,[1] with all its favourable circumstances, as great as or greater than in lying-in hospitals.

Whatever may be the exact truth on this point, the history of the Dublin Hospital makes it plain that metria is an awful invader of its beneficent precincts. There is every reason to suppose that its attacks may be sometimes prevented, or their baneful effects reduced in amount. To attain to this supremely desirable object, the efforts of science and art should be laboriously and incessantly directed.

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[1] *Dublin Quarterly Journal of Medical Science*, August 1869, p. 313.

CHAPTER XV.

THE CONSTRUCTION OF A MATERNITY HOSPITAL.

I HAVE no intention to enter upon this subject farther than to express my approbation, generally, of the opinions of several great physicians which I shall cite. The circumstances under which their testimony was given, and its tenor, are very well described by Le Fort in his work on maternities. I shall quote at length his account. Before doing so, I must ask the attentive reader to excuse some confusion which he will observe, and which arises from the indefiniteness of terms. This confusion exists in the original documents, and I cannot mend it. This is of the less importance, because the principles of construction recommended are easily apprehended in spite of the confusion. In several places I have not closely followed Le Fort's rendering of the German, having preferred to translate from the original.

"Before deciding (says Le Fort) on the construction of a new maternity hospital at Prague, the Bohemian Diet thought they could not do better than ask the advice of the physicians most competent in such a matter. Four questions were laid before MM. Oppolzer, Rokitansky, and Skoda of Vienna; Virchow of Berlin; Lange of Heidelberg; Schwarz of Göttingen; Löschner of Prague, and Hecker of Munich. Their answers are given in the *Monatsschrift für Geburtskunde* (August 1864, S. 155).

First Question.—Are the contagious origin and extension of puerperal fever epidemics certain, probable, or possible, according to the present state of science?

Answers.—There is no doubt of the origin and extension of puerperal fever by contagion—(Oppolzer, Skoda, Rokitansky).

In the development and the propagation of puerperal fever, the chief thing is a predisposition producing itself in the individual to diffuse and malignant forms of inflammation; from it alone, without contagion taking place, puerperal disease may result. A local specific infection, that is contagion, first makes its appearance when the epidemic has reached a certain degree, and the contagion a certain intensity of efficacy. On an individual not predisposed contagion may continue to be inert—(Virchow).

Puerperal fever consists, according to the meaning of the words, in a disease of the blood produced by infection with decomposed animal matters. Infection generally comes from without, more rarely is it self-infection. The contagiousness of puerperal fever (by inoculation of specific products) must be denied; on the other hand, inoculation by means of animal or cadaverous poison may well be admitted—(Lange).

Puerperal fever, like hospital gangrene, is produced by noxious effluvia—(Hecker and Schwarz).

I observe that, with the exception of Löschner, who does not venture an opinion, the most illustrious physicians of Germany admit in a formal manner the contagiousness of puerperal fever; I have also already proved it by facts, and we ought now to draw a first conclusion:—Women attacked by puerperal fever should be isolated from others.

Second Question.—Are large lying-in institutions, well arranged for the purpose, admissible; or is it preferable to subdivide them into several smaller lying-in institutions? How large may the latter be?

Answers.—If the construction is well adapted for the purpose, and there is sufficient spaciousness, large lying-in institutions are not specially more disadvantageous than small ones—(Rokitansky, Oppolzer, Skoda).

Virchow declares himself decidedly opposed to a great lying-in institution. Separate hospitals for 800, or at most 1600 labours, should be erected in separate parts of a town, with sleeping apartments for from 20 to 30 pregnant women, and wards for 10 lying-in women at most.

Great lying-in hospitals are not admissible. The smaller the institution, the better in general are the health conditions—(Lange).

The construction of great lying-in hospitals is inadmissible. The smaller such a building is, and the greater the extent of space occupied, the better—(Hecker and Schwarz).

Great lying-in institutions are inadmissible—(Löschner).

Here also there is remarkable unanimity. I believe with Virchow that one might, at great expense, construct a large establishment, admitting of 1500 labours annually, with some safety; but I declare myself energetically in favour of small establishments. The chances of infection are less great there; and if an epidemic, which cannot always be prevented, and for whose possible occurrence it is necessary to provide, were developed by contagion, the immediate evacuation and closing of a small establishment involves less inconvenience, because it does not suddenly put a stop to the succour given to a great number of pregnant women. Thus we arrive at this second conclusion:—A maternity should be arranged in such a manner as to admit of 800 to 1000 labours annually at most.

Third Question.—Is it necessary, during an epidemic, to separate and disinfect, and therefore to erect an occasional house—(Wechselhaus)?

Answers.—Separation of the lying-in houses, conveyance of the healthy pregnant and lying-in women into an occasional house of one-third of the size of the lying-in institution, are necessary—(Rokitansky, Oppolzer, Skoda).

Complete evacuation must take place when diffused infection is established; partial evacuation, with separation and disinfection, in the case of characteristic single cases. A middle building for pregnant women, with two wings, to be used alternately for the lying-in, is the form recommended. At a sufficient distance a wash and disinfection house are to be erected—(Virchow).

Separation is of little advantage; disinfection of course imperative; an occasional house necessary—(Lange).

Even an establishment of small extent requires an occasional house—(Hecker and Schwarz).

Complete stoppage of communication between the different houses is necessary. In case of need, entire separation—(Löschner).

Like the high medical authorities whose opinions I have just reported, I am a partisan of the separation of the sick, the complete evacuation of the lying-in house in case of an epidemic, and of the complete disinfection, by washing of the walls, fumigations, opening the windows, rest during one or two months; but I do not join in their recommendation as regards the necessity for occasional houses, at least if by an occasional house is meant an infirmary for the diseased lying-in women. If by the word is meant an alternating maternity, we come, in truth, upon financial difficulties which are of themselves serious; but, what is more so, is that the measure would be probably inefficacious.

In order that the evacuation into the occasional house should be useful, it must be complete; that is to say, it must comprise all the individuals of the medical service and of the general services—kitchen, wash-house, offices, etc. In fact, if the lying-in alone occupy the occasional house, the different services, the employed, the midwives, the physicians, returning several times daily from the infected house, would bring the disease into the occasional house, and the separation would soon be illusory. To make it real, it would be necessary to have, in some sort, two maternities, with double general services, and consequently to involve considerable expenses, with a view to a utility very doubtful and also exceptional; for, in a well-organised maternity, epidemics may be, and should be, very rare. If there were two similar houses, they ought not to be in proximity to one another, within the same ground, at the risk of seeing the separation remain sometimes inefficacious, the atmosphere being able, at so short a distance, to play the part of means of transport of contagious miasms.

What appears to us indispensable, and we believe that such is the signification of the word occasional house, is an infirmary completely separated, as is the case everywhere in Germany, in Russia, at Copenhagen, in Switzerland, for smallpox; an infirmary to which would be carried the lying-in as soon as they presented an individual and isolated case of puerperal fever.

But how is the want of succour to be supplied, that is brought about by closing the maternity when it becomes exceptionally the theatre of an epidemic?

I had the honour, some months ago, in such circumstances, to propose to the Director-general of Public Aid, and the happiness to find him accept and put in operation what I believe to be the best means to employ: confinement at home by the midwives of the town, at a sacrifice of money somewhat considerable, but of short duration. It is easy for an administrative body to come to an understanding with a certain number of midwives, as to their receiving as private lodgers women coming to be confined, and thus supplying the want of hospital beds. A pregnant woman presents herself at the maternity for her confinement, and the establishment cannot receive her; but there is handed to her a billet, with the address of this or that private midwife, and she goes there to be confined as a lodger at the expense of the administration, which knows how many of these temporary beds are at its disposal; how many are occupied; how many and which are still at their service. We therefore lay down the following conclusions:—

Every maternity should include an infirmary, specially arranged for women attacked by puerperal fever, and placed in a building perfectly isolated.

The maternity should have the double of the beds regularly in use, so that each ward, after having been occupied during the time necessary for the recovery of the lying-in who have been accommodated in it, may be ventilated and remain unoccupied during an equal period of time. The alternation should be established between the left and the right side of the house, or between two different storeys.

When a case of puerperal fever shall have appeared in a ward, after the removal of the patient to the infirmary, and the dismissal of the other lying-in women of the same ward, who are to be kept in a sort of quarantine till their recovery, the ward shall be entirely cleansed.

When an epidemic appears in the maternity, no new lying-in women shall be admitted; the establishment may be evacuated; but in no case shall the confined be sent to other lying-in establishments. After its evacuation the establishment shall undergo an absolute disinfection, a complete cleansing; and shall not be again opened till after a rest of a month at least.

Fourth Question.—May the lying-in house and the occasional house be in immediate proximity, under the same management and administration, or not?

Answers.—The occasional house may be in the neighbourhood of the institution, and under the same household management. Physicians, midwives, linen, etc., must be distinct, and remain separated from the lying-in institution—(Skoda, Rokitansky, Oppolzer).

The medical attendance and the services of the several institutions must be separate, and independent of one another—(Virchow).

There should be the greatest possible distance between the lying-in house and the occasional house. In addition, it is necessary to take care that the medical and nursing departments of the two houses be completely separate. Separate household administration is desirable. A lying-in institution should be isolated and surrounded by gardens. Conjunction with other hospitals or gynækological wards is unpardonable—(Lange).

The different houses should be not only entirely separated from one another by ground, but also in furniture, medical attendance, and services—(Hecker and Schwarz).

The authors whom I have quoted, physicians of incontestable authority on this subject, are, as we see, explicit on all the questions. I join absolutely in their views, except one little modification. The essential point is to avoid all direct communication between the maternity and the infirmary; but I do not see what evil can arise from having the hospital, and its annex, under the financial administration of the same manager; for everything else I am in favour of complete separation.

The special infirmary should be attended by a physician who does not take midwifery practice in the town, and who does not reside in the institution. He should be aided by an assistant lodged in the house, but who must not, under any pretext, enter the chief maternity. The same rule will apply to the servants. The infirmary linen should be washed in "its own wash-house."[1]

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[1] *Des Maternités*, p. 313.

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This, the latest and smallest of the volumes which Dr. Duncan has published, is in some respects the most important. The profession is already indebted to him for correcting many errors, clearing up many points which were obscure, and making many crooked paths straight, in connection with that department of medical science which he has cultivated with so much enthusiasm. But we know of no subject in the whole range of Gynæcological studies which is of greater interest, and none whose elucidation is of deeper importance than that which forms the title of the little work now before us.

Upon the whole, we regard this volume as a very considerable step towards clearer, preciser, and fuller knowledge of a group of diseases which yet need much more investigation.

Dr. Duncan by his labours is doing something to hasten that wished-for period, and we would conclude this somewhat sketchy notice of his interesting little book by cordially commending it to the attention of Gynæcologists.

From the European Mail.

This is a most useful little treatise on certain maladies to which women are subject. The author, already favourably known by his

"Researches in Obstetrics," lays claim to no infallibity, as writers upon such subjects so often do; but plainly avows "his indecision on many points," and admits that the opinions he entertains "are very far from being regarded by himself as final." This will by no means diminish, but will rather add to, the value of Dr. Duncan's book, in the estimation of the profession. We refrain from referring particularly to the views which Dr. Duncan enforces in reference to the diseases of which he treats, as a discussion of such a nature is fitted only for the columns of a medical journal; but we generally note the learning and the patient observation which Dr. Duncan has brought to bear upon a difficult department of medical science. Dr. Duncan's nomenclature is new. Taking example from the heart and other organs, he uses the prefix *peri* to denote inflammation of serous membrane; and *para*, inflammation of cellular or connective tissue. This is a very convenient mode of expressing these differences, and it would be well if the profession generally were to sanction its adoption.

From the Athenæum.

To most readers, even professional readers, the subject of this book, as indicated by the title, would be dark and mysterious. But Dr. Duncan is too well known as a labourer in the field of obstetrics to lead any one to doubt that he has chosen judicious terms to indicate the diseases he wishes to describe and treat. These diseases are outside ordinary experience, and can only be described by the practitioner of large experience and acute observation. We cannot here enter into any detail of them; but those who are interested in saving the most precious life in the community—the life of the mother who has just given birth to offspring—will best understand the value of a treatise devoted to diseases which are the result of that condition. We notice this work to say that we think it deserves the attentive study of all engaged in the treatment of those diseases to which it refers.

From the British Medical Journal.

The appearance of this monograph on a most interesting and important subject is well-timed, and a careful perusal of it has led us to the belief that the work will contribute materially to the elucidation of a somewhat obscure piece of pathology.

On the treatment of these affections Dr. Duncan has some sound practical remarks. He has no faith in mercury for the removal of adhesions. Early evacuation of abscesses he recommends. Leeching of the uterus he considers preferable to scarification, though few cases admit of blood-letting in any shape. His book is one which the best informed practitioners will read with interest and advantage.

From the Edinburgh Evening Courant.

The appearance of another work from the pen of Dr. Duncan will be gladly welcomed by the medical profession, for he is already recognised as one who knows how to turn to account the vast experience which it has been his good fortune to acquire in the special branch of the profession to which he has devoted himself. An original and logical thinker, a careful investigator, an accurate observer of facts as they present themselves to him, and, moreover, a deep reflecter on their import, Dr. Duncan further possesses the power of expressing his thoughts in clear and terse language, which gives to his writings an additional value, and imparts to them that philosophic stamp which is far from being the universal characteristic of medical works. Nor does the present volume fail to sustain the reputation of its predecessors.

Believing that much remains to be done before the diseases, to which the title of this book refers the thoughts of the physician, can be said to have arrived even at a sound elementary condition of scientific progress, Dr. Duncan looks forward with hope to see a beginning made of a truly scientific clinical treatment of the matter—of such a scientific treatment as has already overtaken several other sets of diseases. To help on this most desirable end is the object of his work. And so we heartily commend its perusal to our medical readers, referring them for a detailed criticism to more legitimate sources than the pages of a newspaper, where any such remarks would, to the majority of our readers, be as little edifying as they would be devoid of interest.

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He follows his denunciation of rash practice with an exposition of his own, in which he lays the greatest stress on leeches, rest, and poultices. As to mercury, he has discarded his faith in the anti-adhesive virtues of that mineral for the diseases under discussion, and when he administers it, does so in the gentlest doses. Thorough ability, power of getting to the bottom of his subjects, acute criticism, and careful observation, mark the present as Dr. Duncan's former publications.

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From the Lancet.

We owe many apologies to Dr. Duncan for having so long delayed a notice of his most valuable and important work. The truth is, it contains so much matter needing calm and attentive study, that we have been waiting for leisure to read, mark, learn, and inwardly digest the three hundred and sixty pages devoted to the analysis of facts from which deductions of a very remarkable and interesting character, in reference to the laws affecting the reproduction of the human species, have been arrived at. In truth, every one of the numerous chapters into which the author has classified his materials well merits distinctive critical analysis, and we are not without hope of so dealing with certain of them as opportunities may arise. . . .

We have thus endeavoured to convey to our readers a general impression of the characteristics of certainly one of the most interesting contributions to medical statistics which we have ever perused. We are not prepared at the present time to discuss critically many points as to which difference of opinion will arise. Statistics, as we all know, have a name for being convertible according to the fancy of the manipulator; and it might possibly appear on close examination that some of the data used by Dr. Duncan are rather more limited than we should consider safe for formulating laws on the abstruse and complicated functions of reproduction. We say this, however, not with the least intention of depreciating the value of Dr. Duncan's investigations. The want of sufficient data was the greatest difficulty he had to contend with, and the marvel is that he has been able so fully to establish as much as he has done. Not one of the subjects treated but has a peculiar interest for the medical profession ; and we therefore very earnestly recommend the study of the book to our readers.

From the Edinburgh Medical Journal.

Both from the great labour, care, and skill expended in the working out of details, and from the importance of the results, as either new or confirmatory of what was previously known or merely conjectured, the work is one of sterling value. It forms an original and important contribution, not only to obstetric science, but also to the department of political economy which treats of population, and to the principles of life insurance. Within our limits it is not possible to enter fully into all the topics discussed. No justice can be done to the inquiries without a study of the book itself. . . . Within the limits assigned to us, we have been able to give only a very inadequate idea of the rich mine of fact and inference which this volume contains ; yet enough has been said to justify and explain our recommendation of it as a work replete with original and valuable information, the study of which is indispensable to all who are interested in the subjects of which it treats, or who are engaged in similar inquiries.

From the Medical Times and Gazette.

Freely as we have extracted from the stores contained in this volume, we can yet assure our readers that the mine is far from exhausted : we would strongly urge them to read it carefully for themselves. To Dr. Duncan, who is well known as one of the most distinguished Scottish physicians of the present day, we tender, in the name of this Journal, the thanks of the profession for having presented us with a standard work, in which the results of former inquiries in the same departments of knowledge are judiciously blended with a large mass of original matter.

From the Scotsman.

The questions treated of in the volume before us not only affect the health and happiness of individuals and families, but exercise an important influence on the prosperity of states. Dr. Duncan has conducted his researches in a most complete and comprehensive manner, and has furnished us with a contribution to vital statistics most valuable, both from its elucidation of ascertained facts, and from the gaps and uncertainties in our knowledge which it has shown to exist. It supplies us at once with an epitome of all that is known on the subject of which it treats, and places us on a firm basis from which to advance to new acquisitions.

From the Dublin Quarterly Journal of Medical Science.

In concluding this brief review, it is hardly necessary for us to express the high opinion we have formed of the work before us. By its publication Dr. Duncan has rendered an important service to medical as well as to statistical science. He has, moreover, set us a good example how to rightly conduct statistical investigations, so as to avoid the shoals and quicksands which beset the explorer in these regions. We are not speaking too strongly when we affirm that this, in truth, is an original and philosophical work, and gives abundant evidence of deep thought, severely logical exactness, and patient industry. On some of the important questions to which his researches refer, the data are scarcely sufficient in quantity to render the accuracy of his conclusions unimpeachable; but the author admits and regrets this, whilst telling us they were the only data available for his purpose.

From the North British Review.

The book which we are about to review is not a medical work, but a treatise on statistics referring to the topics named in the title. These statistics have been compiled and arranged with much care, and are handled by the author with acuteness and without prejudice. The book may therefore be read with pleasure and advantage by all who take an interest in the physical laws affecting the natural history of man and his social welfare. The book is not at all prolix or dogmatic, for Dr. Duncan belongs to the very valuable class of authors who collect and digest facts, but refrain from the reflections which these facts suggest. We have been tempted to indulge in some speculations, and feel certain that all readers who can think will find new matter for consideration in the book. They will find nothing garbled, no concealment, no prejudice, but a large collection of interesting materials intelligently arranged.

From the British and Foreign Medico-Chirurgical Review.

A valuable contribution towards the study of this subject. Our limits prevent us from undertaking more than a very brief survey of the opinions put forward, and we must refer our readers to the volume itself for the tables on which they are based.

From the Glasgow Medical Journal.

To do sufficient justice in a mere cursory notice to such a work as that on Fecundity, etc., is simply impossible. The subject is a vast one, and comprehensive as is Dr. Duncan's treatise, he evidently does not claim the merit of having exhausted it. It would be difficult to find any one better adapted than Dr. Duncan for the task which he has so ably performed, and whereby he has rendered such valuable service to medical as well as statistical science.